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THE PSYCHOLOGICAL REVIEW

THE NATURE OF PSYCHOLOGICAL 'TRAITS' [1]

BY ANNE ANASTASI

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I do not propose to add another to the many definitions and classifications of traits which have appeared within the past decade. Rather is it my object to inquire into the sources of diversity among trait theories, in the belief that such an analysis will itself help to clarify the problem of traits and to point the way toward significant investigations.

SIMILARITIES AND DIFFERENCES AMONG EXISTING TRAIT THEORIES

First we may ask whether *any* common ground can be found in a survey of existing trait concepts. Reduced to its most elementary terms, a trait may be regarded as a category for the orderly description of the behavior of individuals. Even this simple statement, however, will undoubtedly meet with difficulties. Some may argue over the term 'category'; others will object to 'description'; a few may be alarmed by such a word as 'behavior'; some will resent the *s* in 'individuals'; while a number will give descriptions that are the reverse of orderly!

But if for the moment we keep our eyes firmly fixed upon similarities, even though these similarities may not be universal, we find that most trait theories are concerned with the *organization and interrelationships* of behavior, and are therefore derived from the observation of varied behavior manifestations of the individual. Trait concepts also refer, as a rule, to relatively *enduring* characteristics which thus have some predictive value. In general, they cover those characteristics which appreciably *differentiate* the individual from others. Thus if the observable similarities in a particular mode of response far exceed any individual differences therein, such a characteristic is not likely to be incorporated in current trait schemas. Finally, a *cultural frame of reference* is also evident, although not always explicit, in most trait classifications. It is those aspects of behavior which are significant within a particular culture or environmental setting that are usually identified and described as traits.[2]

From this brief overview of the possible points of agreement among trait theories, we may turn to the less taxing search for their disagreements. The *number* of traits proposed varies from

[1] Presidential address delivered at the annual meeting of the Eastern Psychological Association in Atlantic City, New Jersey, April 25, 1947.

[2] For typical discussions of the concepts of 'trait,' 'ability,' and 'factor,' as well as proposed schemas of classification, *cf.* Allport (1), Burt (11, 12), Carr and Kingsbury (13, 14), R. B. Cattell (15), Guilford (23, 24), Thurstone (38), and Tryon (39). For a recent survey of methods and results of factor analysis, *cf.* Wolfle (46).

a single general factor to innumerable highly specific elements, with the majority of those psychologists who approach traits through the medium of factor analysis favoring a relatively small number of 'group factors' of an intermediate degree of generality. As to *stability*, traits are regarded by some as fixed and immutable throughout life, by others as undergoing a uniform and predetermined course of development with age, by still others as the constantly evolving resultant of the myriad influences which play upon the individual. Similarly, in the degree of *universality* of the proposed traits, theories vary from an implicit belief in a universal pattern of primary human traits, at one extreme, to the insistence that truly significant traits are unique to each individual, at the other. Attempts have also been made, at a highly speculative level, to identify traits with various *anatomical mechanisms*, such as neural patterns or genes. On the other hand, some writers have argued that such identifications are precluded by the very concept of traits as they envisage it. Similarly, traits have been endowed with *causal* properties by some psychologists, while others regard them as theoretical constructs or as points of reference in the classification of behavior.

Descriptive Approach Versus a Search for Principles

It is our thesis that this diversity of trait concepts is the result of an underlying methodological limitation which has characterized not only trait studies but many other types of psychological investigations as well. The trait investigator has usually asked: *"What* is the organization of behavior?" or *"What* are the traits into which the individual's behavior repertory groups itself?" rather than asking, *"How* does behavior become organized?" and *"How* do psychological traits develop?" Much of the content of psychology—including trait theories—still consists of generalized factual descriptions rather than principles of behavior.[3] It represents a cataloguing of responses within a specific (although not usually specified) setting, without reference to the conditions which bring about such responses. Within the typical textbook, for example, one finds an account of how the individual looks and acts—and possibly how he feels—when angry or afraid or elated; or a discussion of the stages through which the child passes in learning to climb stairs, pick up objects, or talk English; or a compilation of the drives which motivate behavior; or a listing of primary mental abilities.

Such content has been criticized by students of comparative culture as being 'community-centric' (34). It has been argued that textbooks of so-called general psychology are in large part textbooks of the 'psychology' of people in western Europe and America. A good portion of their content might in fact be characterized as the psychology of "American college students and urban school children." When emotional expressions, or stages of child development, for example, are checked in different cultures, the results are quite dissimilar to those reported in the typical text which purports to cover human behavior. The writings of many anthropologists and social psychologists furnish a rich source of illustrations of such behavior divergencies in different cultures (*cf., e.g.,* 26, 34).

The factuo-descriptive approach is at once too narrow and too broad. From the viewpoint of comparative culture, its findings have proved to be too narrow. The clinical psychologist, on the other hand, often complains that it is too broad for his purposes. This is the

[3] *Cf., e.g.,* the discussion of this point by Foley (18, 19).

basis for the aversion to what has been inappropriately labeled the 'nomothetic' approach. By definition, a nomothetic approach is one which seeks fundamental laws and principles. The clinician objects to so-called nomothetic generalizations because they yield only crude approximations of the individual cases with which he must deal. But it is not against the *principles* of behavior that he is actually arguing. It is against the generalized—or perhaps unduly overgeneralized—factual descriptions that his criticism is in effect directed.[4] The truly nomothetic principles of behavior apply equally to all individuals and will in fact explain the very uniqueness of each individual's behavior in terms of the unique combination of antecedent conditions.

It is no real solution to revert to the solitary contemplation of one individual. To do so is simply to substitute an individuo-centrism for the community-centrism of many current psychological generalizations.[5] At best this would only yield information known to be applicable to a single subject, information whose usefulness would quietly expire with the demise of that privileged individual. But actually, if followed seriously and faithfully, such an individuo-centric method would not even furnish that much information. It is doubtful whether any understanding of such an individual could be achieved without previous familiarity with at least a few behavior principles which had been discovered under controlled conditions. It is as if a physician attempted to diagnose a case of cancer or appendicitis through a prolonged and sympathetic observation of his patient, but without benefit of any of the experimental findings of physiology, histology, chemistry, and other nomothetic disciplines.

To repeat, the factuo-descriptive approach yields approximate knowledge of relatively restricted groups of individuals. Such knowledge is neither sufficiently extensive—*i.e.*, applicable outside the cultural setting in which it was obtained, nor sufficiently intensive—*i.e.*, completely and precisely applicable to any one individual. Principles of behavior, on the other hand, are at once universal and precisely applicable to each individual.[6] It is only through a search for principles, therefore, and not through anti-scientific revolts and individuo-centric procedures, that we can transcend the limitations of the factuo-descriptive approach.[7]

THE THEORETICAL IMPLICATIONS OF FACTUAL INCONSISTENCIES IN TRAIT RESEARCH

A survey of recent trait studies reveals various superficial inconsistencies, which we would expect to find because of the predominantly factuo-descriptive approach of such investigations. The diversity of trait concepts which have been proposed is closely paralleled by a diversity of factual material collected in descriptively oriented research.

'Intellectual' and 'emotional' traits. In the search for the *conditions* associated with this diversity of results, we

[4] Essentially the same point has been made by Chein (16), who discusses the confusion between 'law' and 'statistical generalization' with special reference to Allport's *Psychology of Personality* (1).

[5] For further critical discussions of this point, see Bills (9) and Guilford (23).

[6] Hull (25) has suggested, for example, that innate individual and species differences may be represented by 'empirical constants' in the equations expressing the laws of behavior.

[7] Most trait concepts, whether derived from clinical observations or factor analysis, provide at best only statements of response-response relationships, which have no tie-up with the stimulus variables and are therefore of dubious value in prediction and control of behavior. *Cf.*, in this connection, the comprehensive analysis of theory construction in psychology by Spence (35).

first come upon a differentiation between studies concerned with 'abilities,' or *intellectual traits*, and those concerned with *emotional traits* or 'personality characteristics' in the popular sense. In studies on intellectual characteristics, the emphasis has generally been on 'common traits,' such as those discovered by factor analysis—traits which are common to large groups of people. Verbal, numerical, and spatial factors are among the now familiar 'traits' which have been identified in various factor pattern analyses. On the other hand, students of 'personality,'[8] concerned primarily with the emotional and motivational aspects of behavior, have more often argued for 'individual traits,' rejecting common traits as unrealistic, artificial, and abstract. Some have maintained that they find it more useful to describe each individual in terms of his own peculiar behavior relationships, rather than to look for common patterns of relationship shared by many individuals. Factor pattern analyses of emotional and motivational characteristics have also yielded results which are less consistent and more difficult to interpret than in the case of intellectual characteristics.[9]

Whether found by factor analysis, type studies, or genetic and biographical observation of a single individual, a trait is always essentially a pattern of relationship within the individual's behavior. The so-called 'common trait,' located by studying a group of persons rather than a single individual, is simply a generalized description of a pattern of behavior relationship which is shared by a group of individuals. Why, then, have such common traits found more support in studies of the intellectual aspects of behavior than in those dealing with its emotional and motivational aspects?

The reason is not difficult to find when we consider the greater *uniformity and standardization of experience* in the intellectual than in the emotional and motivational sphere. An obvious illustration of this fact is furnished by our system of formal education in which the standardized content of instruction is directed towards intellectual rather than emotional development. Furthermore, even if the schools were to institute a rigidly standardized curriculum of 'personality development' (a rather depressing thought!), we still would not find the uniformities of organization characteristic of intellectual development, since much of the individual's emotional development occurs through domestic or recreational activities. Not only educational courses, but also occupations and other traditional areas of activity within any one cultural setting tend to crystallize the individual's intellectual development into relatively uniform patterns. Such patterns become more clearly evident the longer the individual has been exposed to these common experiences.[10]

[8] *Cf., e.g.,* Allport (1).

[9] *Type theories* may be regarded as an attempted compromise between the two extremes of common and individual traits. Such theories imply essentially a pattern of behavior relationships shared by a relatively limited group, narrower than the groups to which the common traits of factor analysis are ascribed. It might be added parenthetically that the use of so-called inverted factor analysis, in which the rôle of persons and tests is reversed in computing correlation coefficients, is not fundamentally different from the more usual procedure of locating traits. Type theorists have enthusiastically claimed inverted factor analysis as their own special method, adapted to the discovery of types rather than common traits. As Cyril Burt (11) has clearly demonstrated, however, the same group factors will emerge whether we begin by correlating tests or persons. Ultimately, both procedures deal with the relationships among the responses of persons to test stimuli. Whether we begin our analysis with tests or with persons will make no difference in the relationships which we find in the end.

[10] A similar point has been made by John E. Anderson (5) in reference to the feasibility of *measurement* within different areas of behavior as well as at different ages.

Those aspects of behavior in which the individual's activities are less standardized in our culture will exhibit a correspondingly more idiosyncratic organization into traits. A further reason for the greater standardization of intellectual responses is the relative degree to which these responses have been verbalized, as contrasted to emotional responses which are more largely unverbalized.[11] It is also relevant to point out that the very distinction between intellectual and emotional aspects of behavior is itself culturally determined.

Nature of subjects. A second major source of diversity in trait research is associated with the type of subject under investigation. We may consider in this connection the categories of age, educational level, occupation, sex, cultural grouping, and species. *Age differences* in the pattern of trait organization have now been quite clearly established. Correlation studies on preschool children show a large general factor, frequently identified with 'intelligence,' with little or no specialization into group factors. Among school children, the weight of the general factor decreases with increasing age, and group factors of memory, verbal, numerical, spatial, and other aptitudes appear. The independence of these factors from each other tends to increase with age. Studies on college students have consistently shown prominent and relatively independent group factors, with only a negligible general factor. The latter, furthermore, is more closely identified with verbal ability among the older subjects. The increasing differentiation of ability and clearer emergence of specialized traits with age has been demonstrated both by the comparison of subjects of different age levels and by the more direct approach of re-testing the same children at successive ages.[12]

Such a developmental approach to trait organization is a step toward the reconciliation of factual divergencies. No longer need we be disturbed by the fact that an investigator who obtains his data from fifth grade school children seems to support a general factor theory, such as Spearman's 'g,' whereas another investigator, testing college sophomores, 'proves' the presence of verbal, numerical, spatial, or other aptitudes which are almost completely uncorrelated with each other. Both are demonstrating the differentiation of traits with increasing age.

In discussions of age changes, however, there is often a tacit implication that maturational processes are involved, that the changes would occur regardless of what the individuals had been doing during those years. This is not a matter for assumption, but rather for investigation. Other group comparisons need to be made in the effort to clarify the source of the obtained age differences. Some of these comparisons have been made, but the available data are not so extensive as in the case of age differences. In fact, the information is often so meager as to do no more than point the way for a type of comparative investigation which should be pursued as thoroughly as has been done with age.

In this connection, we may consider next the influence of *educational level.* It should be noted that nearly all the trait studies showing differentiation with age have also included the variable of education, which increased concomitantly with age: the older groups of children were attending higher grades in school; the adolescents and adults

[11] A point made as early as 1924, by John B. Watson (44, pp. 130–131).

[12] For a comprehensive summary of the data on age differentiation of intellectual traits, *cf.* Garrett (20).

were generally college students, not elementary school graduates.

Some highly suggestive data regarding the rôle of education are furnished by an analysis of the scores on the Army General Classification Test used in World War II. Within a sampling of 5000 men, carefully chosen so as to be representative of the entire army, with a mean age of 27 and mean education of 9½ grades, intercorrelations were computed among the scores on the verbal, numerical, and spatial sub-tests of the AGCT (42). The correlations proved to be decidedly higher than those found for similar tests among college students. But a more significant finding was the relative uniformity of such correlations among all tests, regardless of content. The correlation of the two numerical tests with each other, for example, was not much higher than the correlation between the numerical and verbal, the numerical and spatial, or the spatial and verbal tests.[13] Similarly, the three parts of a Mechanical Aptitude Test correlated about as highly with total and sub-test scores of the AGCT as they did with each other. These results were closely corroborated by correlations obtained on another sampling of 500 men who had taken different forms of the AGCT and Mechanical Aptitude Test (41).[14] Such findings suggest that among adult men in the army whose education was little better than elementary school graduation, test performance was relatively undifferentiated. In this respect, their trait structure thus resembled more closely that of elementary school children than that of the college students tested in the earlier studies on adults. Similarities of educational level seem therefore to be more influential than similarities of age in trait organization.

Further evidence of the relationship between education and trait organization is to be found in a factor analysis conducted with some of the standardization data of the Wechsler-Bellevue Intelligence Scale (7). The average intercorrelation of the ten sub-tests of this scale dropped consistently from .37 at age nine to .18 in the 25–29 year group. In the 35–44 year group, however, it rose to .31 and in the 50–59 year group it rose again to .43. Similar decreases and increases were found in the average contribution made by the first factor to all of the tests. Now, it is significant to note that the educational level of the two oldest groups was lower than that of the 25–29 year group. The latter had a range of education from one to four years of high school; the next older group from the sixth grade to one year of high school; and the oldest group from the fifth to the eighth grade. Thus we again find that among subjects of lower educational level, irrespective of age, abilities are less highly differentiated and the general factor is relatively conspicuous.

Occupation is a third category whose rôle in trait organization may be considered. The data assembled by the

[13] AGCT-3, consisting of (1) reading and vocabulary, (2) arithmetic computation, (3) arithmetic reasoning, and (4) pattern analysis. Correlation between the two numerical tests was .90; correlations between verbal, numerical, and spatial ranged from .71 to .81. The three parts of the Mechanical Aptitude Test correlated .67, .78, and .71 with each other and correlated .77, .76, and .77, respectively, with total AGCT scores. Correlations of the four sub-tests of AGCT with the three sub-tests of the Mechanical Aptitude Test ranged from .65 to .72.

[14] AGCT-1, consisting of (1) vocabulary, (2) arithmetic, and (3) block counting. Intercorrelations of the three parts of the Mechanical Aptitude Test ranged from .60 to .78; correlations of these part scores with total AGCT scores ranged from .58 to .69. When correlations among the sub-tests of the AGCT were computed within subgroups which were relatively homogeneous in total AGCT score, the correlations dropped to low, negligible values. The highest of these correlations, in the 30's, were in fact negative (40).

Employment Stabilization Research Institute of the University of Minnesota, for example, demonstrate clear-cut and consistent differences in the profile or pattern of test scores in various occupations (17). A further question, of more relevance to the present inquiry, concerns the intercorrelations of such scores within each occupational group. Very little direct information on such correlations is available to date. The interpretation of the findings is complicated, furthermore, by the fact that the occupational groups tested are not usually equated for such variables as age and education. Some suggestive data, however, can be found in the University of Minnesota studies to which reference has already been made. Three groups of unemployed men were given a battery including two or three tests in each of the following categories: intelligence, clerical aptitude, spatial aptitude, and manual dexterity (36). The three groups of men had previously been employed as clerks, skilled trades workers, and operatives in repetitive tasks, respectively. Intercorrelations among the tests were fairly similar in the three groups, the averages of all the intercorrelations being virtually identical in all three.[15] A few specific differences can be found, however, which are of interest. Thus, the intercorrelations of the three manual dexterity tests were consistently higher among the operatives than among either of the other two groups.[16] The dexterity tests also correlated consistently higher with tests of clerical aptitude and intelligence among the operatives than they did among clerks or skilled trades workers. On the other hand, the dexterity tests correlated lower with the spatial tests among the operatives and the skilled trades workers than they did among the clerks.

One could speculate regarding the implications of these group differences. For example, perceptual accuracy, closeness of attention, and motor speed may be important common factors in the operatives' performance on the clerical and intelligence tests, as well as on the dexterity tests. On the other hand, the performance of clerks and skilled trades workers on the clerical and intelligence tests may be much less dependent upon such functions and therefore less closely related to their performance on the dexterity tests. Among the clerks, manual dexterity may be largely a function of the amount of manual-mechanical experience they have had, a fact that would link their scores on the dexterity and spatial tests more closely than in the other groups.

It would also be interesting to determine the degree to which the general factor is associated with verbal ability in different occupational groups. Most of the studies conducted to date suggest an increasing identification of verbal ability with whatever general factor is found, as the individual advances in age. Remembering that the older subjects employed in factor analysis studies have generally been college students or college graduates, we may ask: Could not the heavy reliance of higher education upon verbal ability account for this relationship? If we carried out a factor analysis on manual workers, might not any general factor found in such a population be more closely identified with such characteristics as accuracy of observation, perceptual speed, attention control, and the like?

A fourth category for comparative investigation is that of *sex*. The differences in the traditional educational, vocational, and recreational activities of men and women are still sufficiently

[15] .31, .30, and .33, respectively, for clerks, skilled trades workers, and operatives.

[16] Average intercorrelations of the three dexterity tests were: .26, .25, and .41, respectively, for clerks, skilled trades workers, and operatives.

large to be reflected in sex differences in abilities. Sex differences in mean scores in verbal ability and memory in favor of the women and in numerical and spatial ability in favor of the men have been well established on representative samples in our culture. The interrelationships of abilities in the two sexes have not been as fully checked as their respective levels, although intercorrelations and factor pattern analyses on a few comparable samplings of the two sexes are available. There is some evidence to be found in such studies which suggests that the nature and organization of intellectual traits are not identical in men and women.

In an early English study (28) on mechanical aptitude in school children, for example, the boys' scores on the various spatial tests correlated more highly with each other and less highly with estimates of 'general intelligence' than did the girls' scores. On this basis the investigator suggested that performance on spatial tests depends more largely upon special aptitude among boys, while among girls it is more largely influenced by those processes which are included under the rubric of 'general intelligence.' In a more recent study (29) on 7-year-old English school children, a factor pattern analysis revealed a spatial factor among the boys, but not among the girls. This author likewise concluded that some of the tests which involved a space factor for the boys were done by the girls "by means of their general intellectual facility."

Corroborative evidence is furnished by studies on American school children at the 9 and 12 year level (32, p. 32; 6, p. 21). In these groups, the correlations of the spatial tests with verbal and numerical tests were higher among the girls than among the boys. In the same groups, the intercorrelations of verbal tests with each other tended to be higher among the girls, while the intercorrelations of numerical tests with each other were higher among the boys. These sex differences in intercorrelations were more clearly apparent in the 12-year-old than in the 9-year-old groups (*cf.* 6, p. 21). Similarly, a factor pattern analysis of the performance of English secondary school boys and girls on a battery of verbal and numerical tests yielded factors which differed in number and location in the two sexes (10).

Studies on memory, conducted with school children of three age levels, from 9 to 15 (21, p. 21), as well as with college students (2, p. 39; 3, p. 15), yielded evidence which suggests that memory operates more nearly as an independent 'trait' among women than among men. Thus the intercorrelations of memory tests with each other tended to be higher among female subjects. At the same time, the community between memory and non-memory tests tended to be greater among male subjects.

Such data indicate the possibility that those groups which excel in performance within a given area, such as spatial or verbal aptitude, exhibit a more closely knit organization of performance within that area. Thus, in the studies cited, the women excelled in mean scores on the memory tests and also yielded higher intercorrelations among such tests than did the men. In mechanical aptitude, the boys excelled in score and also showed higher intercorrelations among the mechanical tests than did the girls. Furthermore, the correlations between spatial and non-spatial tests were lower among boys than among girls. Might this not suggest that the very conditions which make for good performance along certain lines tend also to crystallize and unify such performance into a clearly defined 'trait'?

A fifth category we may consider is that of *cultural groupings*. Relevant data are virtually non-existent in this

area, the principal reason undoubtedly being the difficulty of devising a battery of tests applicable to widely diverse cultural groups. Although attempts have been made to devise tests which are relatively 'culture-free,' such tests do not offer sufficient variety and breadth of content to permit factor pattern analyses of the scope of those conducted within our culture. An analysis of the intercorrelations of scores on even the limited type of tests available would, however, be illuminating. It would be very surprising indeed if in cultures very unlike our own we should find the emergence with age of the verbal, numerical, spatial, and other familiar 'aptitudes' of our factor studies. To be sure, a certain degree of differentiation into relatively unified behavior 'traits' might occur with age within the initially amorphous and homogeneous pattern of behavior. But the nature of such traits and the degree of differentiation would probably be most unlike those found within our culture.

Turning to *species*, our sixth and last category, we may inquire what investigators on infrahuman animals have to offer to the concept of traits. A number of factor pattern analyses have been made, most of them on white rats (*cf., e.g.*, 8, 22, 27, 31, 37, 43, 45), but their results have all but defied interpretation. In one study (27), for example, the intercorrelations of performance in nine tests were so low and insignificant that nothing could be concluded beyond extreme specificity of behavior. As for the rest, the interpretations of the 'factors' are highly speculative. Most of these factors were limited to a particular type of situation or learning problem. Some have been defined in terms of specific techniques which the animal may use in solving more than one problem, such as the principle of turning alternately right and left (37, 43). One is impressed, furthermore, with the frequency with which factors related to the emotional aspects of behavior appear in conjunction with 'intellectual' factors and with greater prominence than the latter. Thus we find in the factor descriptions such terms as "a combination of intelligence and tameness," "wildness or panicky behavior," "wildness-timidity," and "self-confidence."

The difficulty of identifying factors in these infrahuman studies, as well as the intertwining of 'ability' factors and 'emotional' factors, is not surprising when we consider that white rats have not been subjected to formal education with standard sequences of courses in Elementary Maze-running 1–2, Problem-solving 5–6, or Advanced Seminar in String-pulling. Unlike the school children or college students of the human factor studies, white rats have not experienced that classic dichotomy between curricular and extra-curricular, between standardized intellectual development and unstandardized emotional development. It would seem that genetic studies of the *development* of factor patterns in animals living under controlled laboratory conditions might be the most fruitful contribution that infrahuman investigations could make to the concept of traits.

Experimental development of traits. The comparison of trait organizations found among the different categories of subjects which we have considered points up the need for a more direct investigation of the mechanism by which traits develop—the way in which the specific experiential background of different individuals determines the organization of their behavior into more or less unitary and stable 'traits.' A preliminary, exploratory study of this question was conducted by the present writer about ten years ago (4). Through the interpolation of a relevant experience, the pattern of relationship

among five tests was experimentally altered in a group of 200 sixth grade school children. The experience consisted of the acquisition of special techniques which would facilitate performance on three of the five tests employed, thereby introducing a new bond among these previously unrelated tests. An interesting parallel in an everyday life situation is furnished by a study on the organization of mathematical ability in English school children (30). Wide variations in the correlations among arithmetic, algebra, and geometry test scores were found in different school classes, and these variations were related to such conditions as whether or not these subjects were taught by the same teacher or whether the methods of teaching them emphasized similarities of techniques.

Also relevant are studies on the effect of practice upon factor patterns. Woodrow (47, 48) found, for example, that after prolonged practice, marked changes in the factor loadings of tests occurred. Nor were these changes a matter of greater reliance upon speed or upon general ability after practice, as might have been expected. Specific changes in the factor composition of most of the tests occurred in the course of practice, with no evidence for the increasing rôle of speed or general ability.

Such experimental approaches to the development of traits open a way for exploring the mechanism whereby the trait organizations found in the purely descriptive, 'cross-sectional' studies may have developed. The accumulated effects of education, occupation, and other everyday life activities upon behavior organization may be illuminated by a study of the condensed effects of short-range experimentally controlled experiences.

It has been argued (*cf.* 38, p. 210) that in such experiments what is changed is the *method of performing* the tasks set by the tests. Such an explanation is indeed plausible, but it should be invoked consistently. For example, when the test scores of subjects of different ages, occupations, or educational levels show diverse patterns of 'trait organization,' such differences too may be explicable in terms of different methods of work. Similarly, any uniformity in factor pattern among members of a given population may in part be the result of commonly acquired methods of work. Factor pattern analyses reveal only the organization of performance as it is found in a group of subjects, but do not indicate the etiology of such organization. If we grant that the 'traits' discovered by factor analysis are simply 'functional groupings' observable within the subject's behavior (*cf.* 38, p. 190), then such traits cannot at the same time be conceived as underlying abilities which remain unaffected while the subject's methods of doing a task and his objectively observable behavior are profoundly altered (*cf.* 38, p. 210). Even the common assumption that certain ultimate limits of performance are imposed by each individual's sensory, neural, and motor equipment must be modified in the light of the possible variety of work methods. Changing the method of work may in part overcome some of these physical limitations and thereby permit the individual to surpass his previously established 'capacity level.' [17]

In summary, it has been the object of this paper to demonstrate that the diversity of trait concepts as well as the apparent factual inconsistencies in trait research result in large part from a predominantly factuo-descriptive approach. It is suggested that the greater consistency and ease of identification of

[17] For a lucid analysis of the rôle of work methods in individual differences, *cf.* R. H. Seashore (33).

traits in the intellectual as contrasted to the emotional aspects of behavior illustrate the greater cultural standardization of activities in the former category. The comparison of factor patterns among subjects differing in age, education, occupation, sex, cultural grouping, and species contributes towards an understanding of the conditions under which traits develop and presents a fruitful field for future research. A more direct approach is the experimental manipulation of behavior organization through the interpolation of relevant controlled activities. The problem of 'traits' is seen as but one illustration of the need for a more active search for the underlying behavior principles which unify the superficial divergencies of the descriptive approach.

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MECHANOMORPHISM: A NEW TERM FOR AN OLD MODE OF THOUGHT

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In psychology the term 'anthropomorphism' has come to mean the interpretation of animal behavior in terms of concepts and processes characteristic of human minds, or, more simply, the ascription of human characteristics to the animal. I do not know when this term with this meaning came into psychology but it became popular at least after the early extravagant use of the anecdotal method by Romanes. At the same time a contrary movement in psychological theorizing took on an accelerated tempo, that which culminated in the rise of behavioristic psychologies and in the construction of mechanical analogies of trial and error learning, conditioning, rats and sowbugs. Enthusiastic over their success, some of the builders and mechanics of these gadgets have embraced only physico-chemical principles of explanations for both human and animal behavior. They have somewhat cavalierly relegated all other explanations to the ash pile of outmoded, anthropomorphic and mentalistic hypotheses of an antiquated medievalism. In the resulting controversy there has frequently occurred the thought that some element was lacking and that this element was some sort of name, label or cognomen for the anti-anthropomorphists. The name should serve to identify the group and its members, to conceptualize their mode of thought, to perform, in short, the same sort of service (or disservice!) that the name anthropomorphism has performed for the anthropomorphists. The label should be one which means the ascription of mechanical characteristics to the human individual, and the interpretation of human behavior in terms of concepts and processes characteristic of machines. This paper suggests the term *mechanomorphism* to carry this meaning. I am not sure that this term is in the dictionary (though 'mechanomorphic' is), but it should be. It lends itself readily to the meaning intended and permits all the variations that anthropomorphism does, *e.g.*, mechanomorphic and mechanomorphist.

One or two examples of the mode of thought described by the term mechanomorphism will assist in defining its characteristics and limitations. In the conclusion of his paper on 'Mind, Mechanism and Adaptive Behavior,' Hull asks this question: "What of consciousness, of awareness, or experience—those phenomena of which the philosophers and theologians have made so much and upon the priority of which they are so insistent? An inspection of the postulates of the miniature system of adaptive behavior presented above certainly shows no trace of any such phenomena" (3, p. 30).

Two types of reply may be made to this query. First, it might be suggested that Hull has not presented all the postulates he actually employed in the construction of his system. This is not an unusual procedure. Rarely does the writer of a text state all his postulates. He will state some, but for the complete list the reader will have to study the presentation and add to the explicit postulates those that the system implies. Thus in considering Hull's system it seems necessary to make a postulate to the effect that there exists a mechanism, call it mind, consciousness

or what you will, that is capable of constructing a system of behavior. This conclusion rests upon the argument from design (2, pp. 81–90) which asserts that the existence of any system, mechanical or otherwise, leads inevitably to the assumption of some mind that created it. A theoretical system of any sort is the result of something outside itself. A theoretical system is not self-generative or self-creative. To find the cause of a machine we investigate the machine for the purpose of getting clues as to the nature of the creating agent. We would not look into the heliocentric conception of the universe to discover its creator. We hunt around for a Galileo, a Kepler, and a Copernicus. So the first answer to Hull's question is that consciousness, mind, and/or experience stand outside his system, not in it. They were the creators of the system.

The second type of answer may be put in the words of a student who remarked (not in connection with Hull's paper) that "these writers seem to do something like this,—they take a pair of words, which in proper English obviously have different meanings, and then, without fair warning, assign to these words almost identical contextual meanings. Thereafter they use these words as synonyms. The result is that what they write seems arbitrary and dogmatic." This comment was made in connection with an author's attempt to reduce the term 'idea' to muscular or neural activity. Is this not what Hull does in his system? Does he not assert a synonymous relation, an identity of meaning, between what appears on the one hand to be a psychological or mentalistic term and a purely neural term on the other? Thus, "Discouragement is the diminution in the power of one excitatory tendency to evoke its normal reaction . . ." (3, p. 16). The student would experience difficulty in identifying discouragement and its presumed neural synonym. Therefore, if one is satisfied with this type of translation, one need not look for mind, consciousness, or experience in Hull's system. (Of course one could argue that Hull is anthropomorphizing the nervous system.)

But why is it that Hull asked his original question? The guess is that he was thinking about a machine, a robot, and not about the conscious human being. As a matter of fact, at the meeting at which the paper referred to was delivered, he demonstrated a mechanical rat or mouse as illustrative of his system. In a word, he was mechanomorphizing.

A more recent illustration, and one that served to arouse the present comments, is given in a paper by Boring (1) on 'Mind and Mechanism.' In that paper he presents a list or inventory of the psychological functions that would be necessary to make a robot a man, or a potato a conscious human being. He admittedly does not have complete success in showing how a robot could be made to do the things he thinks necessary but the implication is that, given time and patience, the thing could be accomplished.

Boring was stimulated to attempt an enumeration of these functions by a mathematician who defied him to "describe a capacity of the human brain which he could not duplicate with electronic devices" (1, p. 178). In response to this challenge, Boring says, "I could not at once name him any, and I confess that I myself thought it would be salutory to show that all human mental functions can have their electric analogues. I lacked, however, an inventory of the functions and thus could not be sure that there was not some psychological function left over, one which a nervous system could perform and an electronic system could

not" (1, p. 178). In making an inventory of these functions Boring set himself the following guiding principles: "The properties are to be expressed objectively in terms of stimulus-response relationships, and the way to keep ourselves clear and to avoid vagueness is to think of the organism as a machine or a robot. . . . With what properties must a robot be endowed by its maker in order that it may make discriminations, may react, may learn, may use symbolic processes, may have insight, may describe the nature of its own functions and processes?" (1, p. 177).

In connection with his completed inventory Boring says that the evidence of its completeness "must lie in the inability of others to confront me with new functions" (1, p. 190). Psychology must be in a sorry condition if no capacity of the human brain which cannot be duplicated by electronic devices springs readily to mind or at least to verbal expression. The following might be suggested:

1. Let one robot construct another, *i.e.*, let it first draw up the necessary blueprints of his proposed structure and then complete its manufacture. (The process of reproduction on a biological level is not here intended, not so much because a robot cannot reproduce but because such a process is not ordinarily considered a brain capacity.)

2. Let the robot write a poem, paint a picture, or carve a bit of statuary. We have machines that will do all of these things on a duplicating or reproducing basis as the printed page bears witness, but only after the poet and the artist have done their share of the task.

3. Construct a robot that will experience the satisfaction of work well done, of something accomplished, such as is experienced by any human worker.

4. Let the robot reflect on its own nature and properties as the psychologist in Boring seems capable of doing.

5. Mr. Boring says he receives a scientific thrill when he feels the returning squeeze of the robot's hand in response to his own. Let the robot arouse in me a thrill,—not the scientific thrill,—comparable to that aroused in me by the returning squeeze of the hand of a fellow scientist.

Some of the functions suggested above involve the concept of creative activity. Has this not been omitted from Boring's inventory? Boring may argue that such a concept requires analysis to reveal the basic and fundamental processes involved in the activity called creative. He may argue further that every process exhibited in creative activity can be reduced without remainder to one or more of the functions in his list. The necessity of such an analysis is readily admitted. One such analysis has been made by Spearman (4). Are his principles of Experience, of Relations, and of Correlates included in Boring's inventory? It cannot be readily seen that they are. Neither is it clear that all of the quantitative principles proposed by Spearman have been incorporated by Boring.

We are not given the basis for Boring's omitting these functions from his inventory. Did he think of them, evaluate them, and then reject them, or did he fail even to think of them? It can probably be safely assumed that he thought of them but rejected them on some ground. What was the basis of his rejection? What was the criterion of inclusion or exclusion? The assumption would be that he rejected them because he was thinking of his robot, not of the human being. He was thinking of those activities, and only those, which might conceivably be duplicated by the robot. In this respect Mr. Boring illustrates the mechanomorphic type of thought shown by Hull's earlier paper.

No one would deny that such a set of activities as Boring proposes would, if built into a machine, result in an interesting gadget, but it is just as certainly

true that such a gadget would never be mistaken for a conscious human being. If one starts with a machine, one ends with a machine. Whatever be the theoretical difficulties in specifying the differences between a machine and a human being they are there. To think of the organism as a machine is to adopt a premise and a method that leads the investigator into a blind alley—a blind alley that precludes the observation of certain types of evidence clearly indicating the presence of activities or capacities which are included in our concept of a conscious human being. Consider this statement from Boring: "You know about a perception, a thought or a motive in another organism by what that organism does, by the way its muscles move and the conditions under which they move, or the way its glands secrete, or the way its nerves generate differences of electrical potential" (1, p. 174). Mr. Fulbright suggested that Truman resign. Did that mean that Mr. Fulbright actually wanted Mr. Truman to resign or did it mean something altogether different? If what the organism does carries its meaning then there should have been no debate, as there certainly was, over what Fulbright meant. You may learn something about a thought or a motive in another organism by observing what that organism does, but you cannot learn everything, perhaps not even the important thing about it, on that basis. The attributes of conscious experience, creative activity, and meaning have not been and cannot be duplicated in a machine.

Mechanomorphism is thus a strictly limited mode of thought. Anthropomorphism had its dangers. But whereas anthropomorphism did not exclude these higher mental processes, mechanomorphism does. If we follow the mechanomorphists we will have to confine our efforts as psychologists to the relatively barren study of reflex action and leave the more important problems to others.

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PSYCHOANALYSIS AND THEORIES OF LEARNING [1]

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Thus far the major result of efforts to bring psychoanalytic psychology into relationship with 'academic' psychology has been to extend the scope of the latter. It does not appear that much, if anything, has been contributed toward solution of systematic problems, or that 'academic' psychology has significantly modified even the scope of psychoanalysis. Differences between the two areas in conceptual orientations, and complexities and divergences within each tradition, are barriers against which efforts to achieve helpful relationship have broken. But it may be that these barriers could be overcome if, instead of attempting to deal either with the total or with secondary features of one or both psychologies, a series of fundamental systematic themes could be drawn from one of them in the terms with which the same themes are treated in the other. This strategy has been applied in the study here reported. An endeavor has been made to discover the major direction of psychoanalytic evidence and theory with respect to the general nature of learning, as that topic is currently conceived in 'academic' psychology. The character of the literature enforces limitation of the inquiry to concern with general principles only. But review of articles and books concerned with learning will convince any reader that the all-important issue does not have to do with completeness of theoretical elaborations; the problem is rather to determine which general principle guides further research in the most profitable direction (5, 30).

There is obviously a major bi-polarity with respect to guiding principles in current theories of learning, a division which Leeper (30) aptly characterizes as 'peripheralist' *vs.* 'centralist.' Probably the most currently influential formulation of 'peripheralist' theory, and certainly the most detailed, is given in Hull's classic *Principles of Behavior* (24). In Hull's conception, learning is fundamentally "the acquisition of receptor-effector connections" (24, pp. 68, 84). The basic principle, from which implementing postulates and corollaries are derived, is that when a stimulus and the response to it are followed closely by reduction in a drive (need), connection tends to be established between the stimulus and the response. This principle, which is called the *law of reinforcement*, may also with no serious misrepresentation be called a *law of effect* (24, pp. 71, 81). The theory has been characterized as 'neo-Pavlovian' (29) since it builds upon, although in the need-reduction hypothesis it modifies, the older 'conditioned-response' theory. 'Peripheralist' theory as characterized above, and presumably in any other formulation, does not deny the reality and importance of organized central processes, but regards this organization as derived from and secondary to repetitions of associated drive-reduction and stimulus-response events, or (in older theory) of stimulus-response events only (33).

[1] This paper was written during residence, made possible by a sabbatical leave of absence, in the psychological laboratory of the Department of Medicine of the University of Chicago, and was read before the Staff Seminar of the Division of Psychiatry. The writer is much indebted to Professor Ward C. Halstead for general advice and assistance, and for the opportunity to study the manuscript of his monograph now in press (19).

The 'centralist' approach to the problem of learning is given in various terminologies (5), but no matter how expressed, embodies as the basic thesis the *primacy* of organization. Learning is held to begin with, and to be explicable only with reference to, central, systematized, processes, whether conceived in terms of brain neuro-physiology or in terms such as insight, interpretation, or knowledge. Repetitions of stimulus-situations or of trials in the learning process may facilitate central patternings, or meanings; they may under certain conditions tend to ". . . establish a knowledge of the properties of the situation" (30). The philosophical implications of the 'centralist' position have been most fully explored by Köhler (28), Koffka (27), and others who participated in development of Gestalt psychology. The core of the argument is that organization (Gestalt, structure, system) is the 'given' reality in nature, occurring not merely as generality of relatedness, but rather in structurally distinguished regions (Gestalten) within larger, more weakly structured, fields of stresses (27, p. 42; 28, p. 31). In elaboration of this general theory, dynamic properties obtaining in the biological realm are held to result inevitably in trends toward expansion and increasing internal complexity (differentiation or individuation) of the behavior of organisms through appreciable periods of time (28, p. 66).[2] *Learning*, then, would refer to the way in which behavioral re-structurizations of some durability occur in consequence of experience with environment. Since these re-structurizations are central processes, a useful clue to the 'centralist' viewpoint is the idea of problem-solving. From Gestalt terminology and broader theory, the term *closure* has come to be rather widely used to refer to centrally controlled instances of learning, and the term *insight* to refer to the psychic manifestations of closure (21, pp. 184–188). Usage seems to justify the convenience of using the term *insight* throughout this paper wherever it appears that, from evidence reviewed, interpretation or meaning is a necessary and prior condition for an instance of learning.

Because of the origin of psychoanalysis from study of the psychoneuroses, clues to learning theory are most likely to be found, in psychoanalytic literature, in connection with instances of non-adaptive fixations. But learning is only the means by which present experience becomes to some extent bound, so that something remaining enters into determination of future behavior. The result may be either non-adaptive or adaptive, depending upon circumstances other than the nature of learning as such. Learning is also as much exemplified in fixation of an avoidance-reaction to a given object as it would be in the opposite case. The axial theme of learning theory is most readily fol-

[2] The philosophy of science barely pointed to above in two sentences cannot here be elaborated. But the writer cannot forbear remarking that, so far as he is able to see, valid criticisms of the philosophy must be predicated on grounds other than charges of either lack of 'objectivity' or equivalence to vitalism. Koffka (27), Köhler (28), and many others have more than adequately met the latter accusation. Koffka (27, p. 308) makes a very convincing case for the argument that Gestalt Theory is the means for *escaping* vitalism; that science must begin with something 'given,' and if it begins with elements or parts—as is the case in the atomistic-mechanism prevailing in 19th century science,—it finds no way to account for organization without the introduction of something not contained in the material with which it began. Gestalt psychology is certainly experimental, both in origin and consequences. Hence if 'objective' refers either to experimental application or to freedom from vitalism, the charge of non-objectivity does not apply. Certainly no soul-stuff, entelechy, or other *deus ex machina* is implied.

lowed in psychoanalytic material by using as a point of departure the evidence adduced and related theory concerning *etiologic traumata* (15, p. 112), *i.e.*, those events or circumstances which may fixate reactions causally fundamental to the psychoneuroses.

It is unnecessary to the inquiry to criticize the Freudian theory concerning maturational stages of instinctual development, or the related theory of polymorphous infantile sexuality (14). For the present purpose, instinctual impulsions may be equated to primary drives, or needs, which are parts of the physiological ground for learning, and may be modified by learning. From either a 'peripheralist' or a 'centralist' point of view, they do not as such, or originally, arise from learning. The intricacies of the defense-processes are likewise beyond the scope of the study; they are complex features of total ego-function, or economy, and while no doubt developed in part from learning, do not readily reveal the nature of learning.

The inquiry is necessarily in the area known as ego-psychology,[3] and the term *ego* will be used only with the broadest meaning which Freud in his later work gave it, *viz.*, as designating *organized* behavior in contrast to the (at least relatively) unorganized energic base, or *id*. (12, p. 30; 16, pp. 29–30). As so defined, *ego* does not refer to a distinction in terms of degrees or levels of consciousness, nor does it refer merely or necessarily to moral development (animals have ego but may lack super-ego [16, p. 30]), nor to any of the various non-psychoanalytic concepts of self or self-regard.

I. Descriptive Classification of Etiologic Traumata[4]

A. *Associated, or coincidental, events.* The specific causes of hysteria were at first believed by Freud to be isolated experiences ". . . which cause fright, apprehension, or psychical pain . . ." and which because of their intensity fixate various responses included in or associated with the original experience, which also from its painfulness is forgotten (15, p. 26). Even in this early paper there are qualifications of the concept of associative connections, such as the possibility that connections might be symbolic in origin. But cases cited are excellent material for a Pavlovian conditioned-response theory. Thus, during an anxious and tragically terminated vigil at a sick-bed, a young woman maintained a position such that her arm became numb; the apparent consequence was hysterical paralysis and anaesthesia of the arm (15, p. 26). But within four years further evidence had compelled Freud to abandon wholly this 'conditioned response' interpretation, so that in cases such as the foregoing the apparent association would be regarded as at most a fortuitous feature of a precipitating circumstance of a type by no means necessary or usually found.[5]

[3] As Mowrer (33) at the close of his paper may mean to suggest, the central problems in ego-psychology involve or perhaps develop immediately from the question of how learning can ever be anything but non-adaptive. From a study of Professor Halstead's manuscript (19) and some acquaintance with the experimental procedures he has used, the writer is convinced that the work provides a basis for solution of this problem.

[4] An early theory which Freud never abandoned, but which need not be reviewed here, is that conditions called 'actual neuroses' are basically caused physiologically from toxins, 'noxiae,' due to disturbances in sexual metabolism set up by sexual deprivation in adult life. These toxic conditions he believed to be also contributory in many cases to precipitation of psychoneuroses (15, pp. 104, 145).

[5] Lay opinion has remained more or less 'fixated' on the early 'conditioning' theory, and the fixation is fostered by fiction, scenario, and even some textbooks in psychology. Excellent material, from which some textbook illustrations have even in recent years been

B. *Seduction in childhood.* The second etiologic theory, which destroyed the first, was presented in 1896 (15, pp. 148 ff.). It was the "notorious theory of infantile traumata" (2, p. 69), according to which the necessary etiologic factor for hysteria and obsessional psychoneuroses is sexual seduction in early childhood. In presenting this interpretation, Freud reported that in each of thirteen hysteric cases there had occurred, according to evidence obtained from patients, ". . . a passive sexual experience before puberty . . . involving . . . actual stimulation of genitalia . . . by adults or older children." The traumata were clearly seductive, not painful or shocking, and their effect was to mobilize sexual impulse prematurely and to tend to fixate it upon the person and situation involved in the seduction. In the cases reviewed, seductions had occurred in most cases before five years of age, and in one case as early as two years of age (15, p. 149).

But in the seduction theory Freud still had not gone deeply enough nor far enough back into infancy. In a paper published in 1905 (15, pp. 276–279), he reported the discovery that the seductions 'remembered' by his patients were for the most part fantasies. From this paper, as well as later ones (17, p. 324; 9, p. 103), it is evident that Freud meant, or came to mean, that the fantasies were operative at the time of traumatization, in childhood, and that whether or not a real seductive event occurs is in itself of no predictive significance whatever (17, pp. 323–324). Investigation of the childhood history of normal persons ". . . yielded the unexpected discovery that their infantile (sexual) . . . history was not different from that of the neurotic, and that seduction had played the same part in it" (15, p. 279). Moreover, the fantasies of traumatic significance are operative in the childhood of all, normal and neurotic alike (12, p. 131; 17, p. 324). The fantasy theory was maintained throughout all later work (16, p. 66), but since the infantile fantasies are universal, they obviously do not, without further analysis of conditions, account for the neurotic traumatization of some children but not others. Something of the preceding seduction theory was retained in the general belief that a factor of possible etiologic significance may be excessive gratification, or indulgence, of infantile impulses, especially as tending to fixate such impulses upon the mother (9, p. 228; 11, p. 164).

C. *Frustration.* As early as 1900 (13, p. 221) Freud was convinced that the major origin of neuroses is to be found in the Oedipus situation, in which the child is motivated by incestuous wishes directed toward one of the parents, but from frustration of these wishes acquires conflicting fears. In general, Freud concluded, it is the frustration of instinctual impulses by social experience which is the major origin of neuroses (11, p. 204). The idea that frustration may play an essential role in fixating conflicting patterns of behavior has certainly been widely entertained by followers of Freud, and has been exploited in many experimental studies in psychology. But, for reasons which will appear more fully later, Freud's treatment of the topic does not easily reduce to simple fixation of a definite response by a definite frustration. The idea is fairly evident (9, p. 269), from the very extensive discussions of the conflicts set up in the Oedipus situation, that frustration of an incestuous wish may fixate a conflicting avoidance-response without necessarily reducing the strength of the frustrated wish. But some discussions

taken, may be found in a paper by Morton Prince written in 1891 (34, pp. 66–91). Here may be found case reports including the expressive phrase, "from that time forward . . ." etc., etc.

are at least open to the interpretation that the frustrated wish may itself be intensified and fixated upon the frustrating or withdrawn object (11, pp. 166–167). The general outcome of this direction of thought is indicated by Fenichel (6, p. 65) in the statement that non-gratification may be fixating, ". . . as if the organism refuses to go further until satisfied." In further summary immediately following, Fenichel suggests some reciprocal relationship between frustration and preceding gratification. If an attempt is made to reduce this to particular frustration-produced fixations of specific responses, the same uncertainty as noted above results. It may be taken to mean that a response which has been strengthened through gratifications becomes even stronger and more object-fixated when it is frustrated even mildly; on the other hand, it may be assumed to mean that when an object-attachment is strong, frustration of it easily establishes a conflicting, presumably object-avoidance, response. It is at any rate apparent that further search must be made to find a theory of learning.

D. *Emotional atmosphere.* Gratifications and frustrations can, of course, be thought of as either isolated events or as chronic conditions. The discussion immediately above has indicated that psychoanalytic evidence places emphasis upon the latter. The Oedipus situation as Freud conceived it is itself primarily a continuing relational context rather than a set, or accumulation, of specific events (9, p. 269).

But the emphasis which came to be placed in psychoanalytic theory upon persisting emotional context is more than merely a shift from emphasis upon isolated events. It involves the concept that in the family milieu unconscious attitudes of adults are absorbed, somehow, into the ego of the child. Ferenczi (7) in 1929 reported finding that the infant senses a rejective attitude on the part of the mother, and responds to it with development of self-destructive trends. Fenichel (6, p. 91) says that specific traumatic events "may be decisive if the Oedipus complex has not been surmounted in a normal way" but advises later (p. 584) that "chronic circumstances in the environment have more influence than any educational measures," and that "the particular words with which a grown-up reacts to the instinctual act of a child are not so decisive as the chronic and latent attitudes of the parent. . . ." Like Fenichel, Hendricks (22, pp. 26, 62g [among the 'facts' of psychoanalysis]) does not exclude possible importance of acute or isolated traumata, but emphasizes the importance of whether ". . . rearers themselves are struggling with unconscious infantile problems." Ribble (36, p. 638) reports carefully obtained evidence that ". . . either conscious or unconscious hostility was apparently communicated in some way" from mother to infant in the first weeks after birth.

Among those who assign most exclusive importance to trends of emotional experience in childhood, rather than to isolated events, are some, notably Adler (1), who in other respects have deviated farthest from psychoanalysis as developed by Freud. More recently, Horney (23, pp. 82–86, 147, 173) has given forceful emphasis to the importance of trend of experience, and has made a logical extention of the concept to the larger cultural context within which the child's immediate milieu exists.

But without pursuing the matter further at this point, it is evident that related to the emphasis on 'atmosphere' there is indication of the operation of interpretation, or insight—of subtle variety—as there was also in the earlier fantasy theory. It is evident also that left-handed concessions of possible operation of isolated traumatic events, as

contributory or perhaps sometimes sufficient, but not necessary, have been carried along inconsistently with the major trend, especially as laid down in the fantasy theory, and that there is confusion rather than clarity with respect to the operation and necessity of gratifications and frustrations.

II. Is There a Psychoanalytic Theory of Learning?

The descriptive classification of possible traumatizing circumstances did not, in the previous section, resolve apparent confusions into a unifying principle. A theory of learning must be based upon evidence concerning factors found to be *necessary*, not merely sometimes apparently operative, in learning. An endeavor to find grounds for a learning theory requires some departure from the more or less historically arranged order in which possible traumata were summarized, and also requires a search for further evidence.

A. *The wreckage produced by the fantasy theory.* The discovery of the operation of fantasies in producing neurosis did much more damage than merely to wreck the earlier theory of specific sexual traumata. Although fantasies operate in part retrogressively, putting meaning back into memories (17, p. 295; 11, p. 167), they are also operative in the original fixation of conflicting patterns of behavior, giving traumatic meaning to the original childhood situations in which such patterns are fixated (9, p. 103; 11, pp. 164, 175). Freud believed, in fact, that the fantasies underlying neuroses are 'phylogenetic possessions,' or racial memories, and that they are operative in the development of all persons (17, p. 324; 12, p. 131). If real childhood events do not supply ready material for 'primal scene' memories (events of incest, threats of sexual injury, witnessing of adult sexual intercourse), the child will utilize such experience as he can find to create such memories from fantasy (17, pp. 323–324; 12, p. 87; 6, p. 91; 22, p. 48). Whether, or the extent to which, real 'primal scenes' have been experienced makes no discoverable difference with respect to neurotic outcome (17, p. 323). Experiences apparently fixating are, it is true, often found, as Freud noted in cases of perversion (9, p. 176), but these experiences are commonplace in nature and have merely been seized upon, or used (9, pp. 176, 214–215). In general, as the foregoing should suffice to show, a careful study of the major trend of Freud's writings in this connection will not support the thesis that childhood 'focal traumata' are in themselves etiologically significant. Neither does it appear that anyone has found evidence contrary to Freud's in this respect. It is of major significance here that fixations of infantile behavior patterns fundamental to neurosis have at no point been found to occur in absence of the necessary operation of interpretation of situations, *i.e.*, insight.

At least one theory of learning has been definitely rejected, *viz.*, the notion of fixation by mere association, or simple 'conditioning.' As Fenichel (6, p. 341) says, the idea of fixation by ". . . a sort of conditioned reflex . . . is . . . a pre-psychoanalytic viewpoint." Even such associative coincidences as may be read into accounts of the determination of content, but not origin, of phobias and fetishes (12, pp. 42–43, 50), are not regularly found nor in any way shown or believed to be necessary factors.

Obviously, the fantasy theory balked the quest for etiologic explanation permitting prediction or prevention of neurotic development. Also, the way was opened for the most elaborate theories of infant mentality derived from biologically carried memories or mind, a direction of speculation most colorfully

developed by Jung (25) and Rank (35). In reporting his discovery of the operation of fantasies, Freud announced that he was compelled to abandon the hope of finding a specific etiologic source of the psychoneuroses (15, p. 279) nor did he later regain such a hope (12, pp. 118, 126). Freud's major efforts remained centered on the nature, rather than differentiating etiologic sources, of neurotic processes, and on the development of a total dynamic psychology (11, p. 207). Insofar as he raised the etiologic question, he emphasized multiplicity of determinants, including phylogenetic, hereditary, physiological, and environmental conditions (12, p. 126). To this there could scarcely be objection were it not for the evident fact that pessimism concerning the discovery of specific etiologic factors included loss of interest in, or pessimism concerning, discovery of how experience with environment operates as even one determinant among others. No matter how important other determinants may be, we still need to know how learning operates: it may be the only condition in the matrix which can be controlled. But, as has been previously shown, throughout Freud's later writings and the literature directly deriving therefrom there may be found passages suggesting that such factors as seduction, gratification, frustrations, and even 'conditionings' can perhaps be effective either singly or in combination, and, in the case of frustrations, in contradictory directions. It is as if the theory of learning were merely that everything can do anything, and sometimes—perhaps—anything can do everything. Whatever may be the importance of this for therapy, it has led to a rushing-off in all directions with respect to prophylactic mental hygiene.

It is hoped that the preceding sketch of historical background will have indicated that this confusion did not arise from any intention on Freud's part to espouse or condone an eclectic position; this he specifically condemned (11, p. 190).[6]

B. *Freud's unrecognized solution.* Underestimation of Freud's genius is an error which many have committed. He created virtually *de novo* the first naturalistic system of dynamic psychology, and he did so under the heavy handicap of the mechanistic philosophy of the nineteenth century. The problem of concern here is one for which Freud had, in effect, given up finding a solution, so that insofar as a solution did appear in his later work, he did not recognize it as such. It is the thesis of this study that from prodigious industry and ability in inquiry and from essential willingness to follow where the evidence led, Freud did arrive at the way to a solution of the problem of learning. Allen (4), among some others, has rather clearly recognized the solution, but to the great majority in the psychoanalytic tradition it seems to have remained obscure.

The clue for solution is Freud's final definition of the ego as organization, or

[6] A careful review of Freud's systematic works will, the writer believes, convince any reader that his remarkable objectivity in reversing his theories as required by new evidence is somewhat obscured by apparent desire to salvage as much as possible from discarded theory, and by the appearance throughout his writing of usages which seem to carry along, inconsistently, remnants of discarded views. These confusions are too much embedded in context to be explicable in terms merely of translation errors. From such usages, contemporary writers who draw most heavily upon Freud's works are the ones most likely to gain impressions which lead them at least to bow in passing to the idea that occasionally a 'conditioning' may be etiologically sufficient. Another very striking instance is the appearance in late works (16) of apparent equivalence of repression and primary defense, in contrast to the clear redefinition of repression as only *one of* the primary defenses (12, pp. 143–146).

system, and as the locus of learning (11, pp. 104–114). As has yet to be shown more fully, no matter in what direction the origin of fixation is pursued, ego or pre-ego organization is found to be prior to and the basis of the fixation-process. It is merely a restatement of the priority of organization or structure to point out that closure, or insight, is found to be always and necessarily operative. This fact which, considering its dramatic first discovery in the operation of fantasies, should have been remembered has been often overlooked, presumably because of uncertainty as to its theoretical significance.

Further marshalling of evidence to show the validity of the generalization made in the preceding paragraph should probably begin with consideration of the child's earliest social relationship. This is an introjective object-relationship called *identification,* or its prototype (14, p. 597). It "has been not inappropriately compared with the oral cannibalistic incorporation of another person" (11, p. 90). "Identification is a very important kind of relationship with another person, probably the most primitive, and is not to be confused with object-choice" (11, p. 90). Since the ego is held to be formed through conscious perception (8, p. 26; 6, p. 75), there is no reason or evidence for referring this earliest object-relationship to reflexive or other non-perceptual processes. It is better described as perception in which the object is not differentiated from inner excitations, a primitive perceptual level to which there is partial regression in dreams (13, p. 349). As Ribble (37, p. 12) expresses it, in infancy "the mother is the ego of the child"—*i.e.,* the object is not differentiated from the perceiving of it or from other conscious processes. It is true enough that in greater or lesser degree the child's physiological needs are gratified by the mother, and that this is a feature of his dependence, but there is no evidence that identification or its prototype [7] is merely an accumulation of, or reducible to, drive-reductive or other stimulus-response connections. It is instead an undifferentiated but insightful perceptual process.

It has been previously shown that the environmental circumstances found to be effective etiologically are the persisting trends and emotional qualities of the social context in which the ego of the child takes on stability of structure. It is, in fact, the unconscious attitudes of adults which are of most importance (7; 6, p. 584; 22, p. 62g; 36, p. 638). Insofar as the child is credited with racial memories and fantasies (17, p. 324; 12, p. 131), an account is available for the sublety of this absorptive process. But another, less genetically improbable, interpretation is at hand. The basic fact is that the infant interprets his psychological milieu from cues which adults do not readily or directly observe. Now, if it is remembered that the great problem for the infant is to establish a relatively stable ego-structure and that the boundary of his pre-ego or ego organization is not firmly drawn (a fact represented in the descriptive phrase 'oceanic feeling' [6, p. 40]), we can readily understand that closure is a vulnerable process, occurring in a highly fluid field. Hence insights are not, as they later become, limited by sharpness of ego-differentiation from object-world, and therefore

[7] The expression 'or prototype' is necessitated by the fact that Freud's interest in the Oedipus situation of later childhood has resulted in usages which may be taken as suggesting the term 'identification' as referring only to a partial re-occurrence of the primitive relationship, by means of which the ego undergoes a unique development in structurization, called the 'super-ego,' from introjection of the parent as a moral and authoritative figure (the super-ego is "a special department within the ego" [16, p. 29] incorporating ego-ideal and self-regard [11, p. 93]).

utilize cues which later would be unavailable or ineffective. For the purpose here, it thus becomes unnecessary to ask whether the child interprets his world in terms of such surprisingly incestuous meanings as Freud postulated (11, p. 164). According to explanation in terms of structure as primary, the fantasies, whatever their content, are manifestations of insight. Since their content would be in some part determined by the nature of physiological needs, if these are as sexual in essential character as Freud believed, the content of insights would of course also be in greater or lesser degree sexual.

In his later work (12) Freud, in the light of accumulated evidence, radically revised his earlier position with respect to the general circumstance which sets defense processes in motion, and in doing so offered further proof of the necessary operation of insight. Whereas his early position had been that repression first occurs and the energy of the blocked motive is released in anxiety, his later position is that the ego 'judges' a situation to be dangerous and creates anxiety, which if sufficiently intense or unmanageable by ego-structure sets repression or other defense processes in motion (12, p. 22). But Freud held always to the thesis that neurotic processes initiated after childhood are reactivations of neuroses of childhood (16, p. 62); it was therefore necessary for him to ask concerning the prototype of the anxiety state to which regression may occur or which may be the state feared in later defense-initiating anxiety. Following his inquiry back toward infancy, one finds that the fear of castration or loss of love, which constitute the apprehended dangers instituting defenses in the Oedipal situation, are, like later anxiety occasions, instigated by interpretations which are seldom if ever justified by actually overt or real threats (12, p. 87; 11, p. 120). Still further back, this remains true, as in the case of pre-Oedipal incestuous fantasies of the girl toward the mother and fear of being murdered by the mother (11, p. 164). Insofar, then, as there may be a prototype in actual injury, it must be, Freud concluded, the experience of birth (12, p. 93). At any rate, the condition which the ego fears or anticipates is a threat to its organization, from a mass of instinctual excitation unassimilable into organization (8, p. 36; 11, p. 117; 16, p. 61). Of most importance here, the only common feature among those situations in which anxiety arises is that they either are, or are judged to be, threats to organizational integrity (12, p. 98; 16, p. 76).[8] The fact that neurotic anticipations are unrealistic, and that the term 'judgment' has been used here to designate "a technique which is, at bottom, identical with normal thinking," but which is 'unconscious' (11, pp. 124–125), does

[8] Anxiety-producing situations obviously include those later childhood or adult experiences which Freud regarded as precipitating, *i.e.*, as being effective only insofar as they reactivate Oedipal or pre-Oedipal conflicts, driving the ego back upon 'retreat patterns' which were established in childhood. Such precipitating conditions may be actual external circumstances or they may be dreams (12, p. 44); they may be observable by others, or unobservable, apparently pleasurable, or—more often—painful (10, p. 324); they may be tempting—mobilizing repressed impulses—or they may be apparently frustrating (9, p. 102). There are some indications in current psychoanalytic literature of a tendency to decrease the sharpness of the distinction between etiologic and precipitating situations (3, p. 9). Insofar as this is done, the situations previously called precipitants would become etiologic, and hence of the same character as those which have been discussed in this paper. Grinker and Spiegel (18), in common with the majority of analytically oriented psychiatrists, regard 'traumatic war neuroses' as psychoneuroses, with precipitants of varied nature as indicated above in this footnote. Kardiner (26) gives greater etiologic significance to the war trauma, and interprets the result as ego-disorganization.

not alter their status as insights, *i.e.*, interpretations.

C. *Secondary factors: gratifications and frustrations.* How can the principle of primacy of structure—organization—cover the confusions in psychoanalytic literature with respect to gratification and frustration as supposed causal factors? How does it occur, or seem to occur, that gratification and frustration, either singly or in sequence, can produce the same result, and that frustration can work either to fixate an object-approaching response or an object-avoidance response?

The answer is suggested by the fact that neither gratification nor frustration is etiologically effective in relation to neurotic patterns except as a persisting feature of an emotional context, or atmosphere. More fully, the answer is that it is not gratification (seduction, need-reductions, etc.) or frustration which is as such the essential condition. In the continuing family situation of the child, these conditions are, when intense and persistent, symptomatic of a prolonged restriction of the child's field of reality, so that there is interference with structural differentiation and expansion of ego.

Indulgence, or excessive gratification, may be indicative of a prolonged binding of the child to identifications which should have been useful only for the beginnings of ego development. The ego is thus prevented from achieving early differentiation from its world, and from achieving those internal structural differentiations necessary for testing reality by means other than attempts at identifications such as appear in the infantile thought processes of dreams (13, p. 349).

The general effect of persisting frustrations is threat to maintenance and expansion of ego-structure. As remarked in the introductory discussion of 'centralist' theory, the fact of dynamic propulsion toward expansion and differentiation is given in the concept of structure as prior in learning. It is therefore not surprising that Ribble (37, p. 1) finds evidence of a need on the part of the infant for environmental stimulation and responsiveness, which she designates 'stimulus hunger.' Levy (31) has applied to the same phenomenon the term 'affect-hunger.' That this requirement is an expression of the "organization of primary energy" (37, p. 3), that is, of organization already present at birth, is shown by Ribble's observation that the consequence of severe deprivation of the infant is not mere lack of ego-growth, but is rather anxiety and trend toward disorganization, which carries threat to life (37, pp. 3–14). Whether the primary organization functional at birth is called 'ego' or 'precursor to ego' is of no consequence here. In either case, organization or system of some kind is primary and has properties of requiring expansion in an introjective relationship to environment and related increase in complexity of structure.

It follows from the foregoing paragraphs that success, failure, punishment, and many other conditions may contribute to the meaning of a situation, as Koffka (27, p. 552) remarks, but they do not operate directly or peripherally as the cause of learning. If the organism is confronted with a situation entirely foreign to his natural and previous milieu, punishments and rewards may well become the clues to meaning which he will attempt to utilize. Since the less the complexity of ego-structure the easier it would be to impose such situations, it may well be that repetition of rewarded responses most clearly appears in experiments with animals (5).

Perhaps the discussion thus far has served to show that no matter from what angle the problem is approached, the learning processes adduced by psy-

choanalytic research and theory will be found to be those requiring a 'centralist' theory of learning. There is no evidence whatever for accumulation or derivation of organization through interlocked series of drive-reductions or other peripherally imposed stimulus-response connections. Neither are the systematic theories and concepts of psychoanalysis consistent with any 'peripheralist' theory.

But it may certainly be asked at this point, if not much earlier, how the lack of evidence for the necessary operation of reward can be reconciled with Freud's early dependence on the pleasure-principle as an all explanatory executive agent. An attempt to indicate the answer to this question is next in order.

III. THE PLEASURE-PRINCIPLE, DUALISM, AND STRUCTURE

Freud at first postulated a single variety of vital energy, *viz.*, libido. Further, he assumed that release of libido is 'pleasurable,' but that accumulation of it prior to or in absence of release is 'painful' (8, p. 1). This view, without Freud's concept of the sexual character of libido, is also fundamental in the contemporary 'drive-reduction' theory of learning (32). It may be useful to restate the postulate as follows: there is 'given' a dynamic force or plan having two properties: (1) a propulsion forward, toward action, and (2) a resistance to forward or outward flow of energy. The latter is implemented by 'pain' in consequence of any accumulation in a given channel of the energy or tension necessary for action. This is, in other words, a dualism which neither Freud nor modern 'drive-reduction' theorists escape.[9] It is greatly to Freud's credit that he recognized this fact.

In making the dualism explicit, Freud stated that it had always been implicit in psychoanalytic theory; motivation has a 'conservative' aspect, as if the organism instinctually 'wishes' both activity and non-activity (8, pp. 4, 67, 71). Other evidences of instinctual dualism—such as sadism and masochism and those subsumed in the concept of repetition-compulsion—need not be reviewed here, although Freud gave them extended attention (8). It is sufficient to note that in the theory of Eros and the Death Instinct, Freud gave explicit formulation to a dualism already present, and that in doing so he also relegated all instinctual forces, *per se*, to the unorganized id (8, p. 79). Although in making his first statement of the theory he admitted the speculative nature of his formulation (8, p. 76), he built upon it throughout later writings (11, p. 141). In this connection most of Freud's followers have preferred to abide by his earlier position, presumably because of some urge toward monism. It may be that a monism is better than a dualism, but the real question at issue for those who use Freud's early theory is whether a smuggled dualism is preferable to an explicit dualism.

But again Freud builded better than he knew, so that he provided the escape from hedonism and dualism, at least so far as a theory of learning is concerned. As has been previously indicated, Freud made no direct attempt to state a theory of learning in relation to his recognition of the dual character of his theory of instinct. Had he done so, it would have been predicated upon his final concept of ego as organization. It is in the ego or the organized precursor to the ego, not in the id as the energic ground, that learning occurs (11, p. 106). The basis of learning lies in the structural

[9] To the writer there seems to be at least a partial recognition of this dualism in the 'drive-reduction' theory of learning, expressed in the Mowrer-Miller Hypothesis (24, p. 278).

properties of the ego or pre-ego organization. At no point did psychoanalytic research 'catch' the organism in the process of fixation without the operation of closure or insight. Freud was much too aware of the trend of his evidence, and much too logically consistent, to assume merely that the infant is an unorganized region within a social field so organized that, taken bit by bit, it can build the organized ego. Instead, he endowed the infant with insights from racial memories (17, p. 325). If we do not wish to account for initial structuredness in Freud's terms, we shall have to do so in terms of a philosophy of science in which system is the 'given' reality in biological nature, as elsewhere.

From a structuralistic point of view, we have no reason to expect that learning can be explained by a pleasure-principle, if by 'pleasure' we mean a feature or property of the physiological sources of energy. These sources, which Freud called the instincts (16, p. 71), supply the raw energy for ego-development and function, but how that energy is used and what qualities it comes to have in learning is largely determined by ego-structure. On the other hand, if by 'pleasure' and 'pain' we mean something ego-experienced, we are dealing with a problem in ego-economics (8, p. 1). Some trends toward closure (some goal-pursuits) are experienced as pleasurable and some as painful, depending upon the balance of structural tensions at work. An analysis of ego-economics with respect to experienced pleasure and pain will be, no doubt, vastly important, but we will find in such analysis only secondary factors of learning (5).[10]

[10] Characteristically, Freud preserved as much as he could of the older pleasure-principle view of ego-function. In his last systematic work, he advises that "the ego pursues pleasure and avoids unpleasure," but "it is probable that what is felt as pleasure or unpleasure is not the absolute degree of the tensions but something in the rhythm of their changes" (16, p. 29).

IV. APPLICATIONS

The purpose in this study has been to discover to what general principle or theory of learning psychoanalysis is committed, in the hope that this may contribute to clearer and more helpful relationships between psychoanalysis and psychology as rooted in academic tradition. Had the purpose been to marshal evidence to prove the validity of a theory of learning, the objection could well be made that however great may be our respect for psychoanalytic achievements, its techniques of research fall short of exact experimentation. But much could be said for the thesis that psychology might very profitably re-examine all of those areas in which research and experience, of whatever nature, have long prevailed in direct dealing with human learning.

It is true that a theory of learning cannot consist of a list of rules for teachers and parents to apply. But such rules should be derivations of theory, taken in relation to social purposes and contexts. It is significant, therefore, that as Allport's discussion suggests (5), our educational techniques appear to be most successful when they are least referable to any 'peripheralist' theory. We have learned in education, but inadequately, to place emphasis upon the 'sense' or structure of subject-matter, and upon the scope of its structure rather than upon routines involving repeated or associated drive-reductions. We have learned, but again inadequately and only after great damage done by applications of 'peripheralist' theory, that the infant needs 'mothering' rather than disciplinary habit-training. We have some glimmerings of an idea that both frustrations and rewards occur in normal development, but that effects are not the ones intended if we try to plan education or child-rearing by attending to or merely controlling rewards and frustrations. A few have learned, but

not from any 'peripheralist' source, that it is the emotional structure of the family, not the externally observable words or deeds, which is fundamental to development of the child, and that the emotional structure of the family is a function of the structure of the culture.

To a considerable extent we are indebted to psychoanalysis, to Gestalt theory and to related developments in psychology for the understandings which have thus far been achieved. We are also much indebted to philosophy, and to accumulated educational and other social experience. But the need remains great indeed for improved educational practices and implementing general principle for learning theory. No matter how convenient for scientific rigor a fragmentary principle may be, it would seem the part of wisdom not to sacrifice a comprehensive bio-social view in our quest for guiding theory.

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THE MEANING OF RIGIDITY: A REPLY TO HEINZ WERNER [1]

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Werner points out that there is considerable confusion about the meaning of the term 'rigidity,' resulting in contradictory interpretation of the same facts. He proceeds to attribute the confusion to some authors who define the term 'structurally' and to others who define it 'functionally,' to some who confuse the concept of rigidity with the concept of differentiation, and to some who assume that rigidity is a uniform rather than a multiform trait. There is, to be sure, a variety of meanings attached to the term 'rigidity.' It is for this reason that the author is inclined to repeat his definition of the concept.

Definition

The construct of rigidity used by the author refers to a postulated property of personality structure that has its place in a series of interrelated statements and constructs in topological and vector psychology. The person is said to be structured and differentiated into parts. The unit of structure is called a region. The construct of rigidity deals with the closeness of the functional relation between neighboring regions of the person.

The published definition (2) is:

"Rigidity is that property of a functional boundary which prevents communication between neighboring regions. The degree of communication of a region A with a region B refers to the degree of influence of A on B, or vice versa. Regions A and B are in communication to the degree to which a change of the state of A changes the state of B."

[1] The concept of rigidity: a critical evaluation (5).

In respect of Werner's statement that some authors assume that rigidity is a uniform rather than a multiform trait, the author has two comments to make. In the first place, the author is not defining rigidity as a behavioral trait, but as a construct. The operational definitions are stated in terms of specific behaviors consequent to particular conditions. In the second place, the degree of rigidity is not assumed to be the same for all regions of the personality structure. There may be differences in the degree of rigidity between different regions of the same person as well as differences in rigidity between comparable regions of different individuals.

The author postulated that the degree of rigidity increases with chronological age. As a corollary, it follows that the degree of rigidity varies with the degree of feeble-mindedness when mental age is equated. On the basis of this postulate, the author, using a hypothetico-deductive method, derived various theorems as to the behavioral differences in prescribed situations between persons equal in mental age and differing in chronological ages. As far as the five predictions that were tested were concerned, the theory was supported.

In respect of this definition, Werner claims that it has "a literal, viz. physical meaning," that it is 'structural' rather than 'functional,' that "the usefulness of this novel definition appears to be doubtful," that "it does not take into account sufficiently the functional dynamics underlying behavior," and that "it leads, in certain instances, to a complete reversal of common sense inter-

pretations by describing behavior in words contrary to their generally accepted meaning."

The author attempted no formulation of the construct in physical terms. If one postulates a hypothetical personality structure it does not mean that this structure is conceived in physical terms. The assignment of a 'physical' property to the construct of rigidity was nowhere stated, intended, or implied by the author. As to whether the construct is 'functional' or 'structural' is a matter of meaning attached to the terms. The author believed he was dealing with a functional concept. It is hard to see how one attaches the label 'structural' to a construct that is defined in terms of the amount of change produced in one region by a change in another. As to whether the "usefulness of the definition appears to be doubtful," as Werner claims, is a matter of opinion. The author can only state that he found it useful for his purposes and that he was able to derive from the construct definite testable propositions. As to whether the construct does or does not take into account the "functional dynamics underlying behavior" is also a matter of opinion. As elaborated in the author's original study, a major reason for explicitly postulating the property of rigidity was to arrive at a genotypical construct in order to account for a variety of phenotypical events. In other words, it was postulated as a functional property underlying behavior rather than as a descriptive concept referring to types of behavior. The author cannot take seriously the criticism that his usage of the term is inadequate because "it leads, in certain instances, to a complete reversal of common sense interpretations." One might ask why so much confusion about the meaning of the term is evident if "common sense" supplies adequate scientific formulations?

BEHAVIORAL RIGIDITY AND DYNAMIC RIGIDITY

It seems to the author that the main difference between Werner's concept of rigidity and the author's is the difference between a phenotypical and a genotypical approach. As a descriptive, phenotypical concept, 'rigidity' has been used in a variety of ways to refer to shorthand descriptions of observable actions. Subsumed actions can roughly be described as stereotypy, perseveration, fixity, lack of variability, pedantry, inflexibility, and the like. This is the descriptive meaning of the term as used in everyday parlance and also constitutes the 'common sense' meaning of some clinicians. As such, it is a phenotypical concept that refers to types of behaviors, and results in classifying some behaviors as rigid and others as non-rigid according to whether the behaviors are perseverative or non-perseverative, flexible or inflexible, stereotyped or variable, and so on. In turn, persons who manifest 'rigid' forms of overt behavior (brain-injured, feebleminded) are labelled 'rigid' persons.

The author was well aware of the above phenotypical usage of the term rigidity and was explicit in differentiating between this general quasi-descriptive term and the construct of rigidity as employed in his experimental studies. In all publications pertaining to his studies he unmistakably stated that 'behavioral rigidity,' in the varied senses of stereotypy, perseveration, fixity, and so on, could not always be used as criteria of the degree of inner rigidity. This lack of correspondence between 'behavioral rigidity' and dynamic rigidity was not made as an arbitrary statement of opinion. Rather, the author pointed out a few other factors that might contribute to behavior of an invariable, stereotyped, perseverative, and so on, nature. Some of these factors

were: (a) Degree of differentiation of the person and of the psychological environment; in this connection a person with a small degree of differentiation behaves in a more stereotyped manner because he has a smaller behavior repertory and has fewer possibilities of action. (b) Degree of differentiation of relevant areas. In this connection, the degree of differentiation is not merely the general degree of differentiation of the person and of the psychological environment but, more specifically, the degree of differentiation that is relevant to the particular situation in which the behavior of the individual takes place. A person with a high general degree of differentiation may behave in an invariable manner in a certain situation because his differentiation is not applicable to that particular situation. He may be highly differentiated and manifest variable behavior in other areas. (c) Miscellaneous motivational factors. In this connection, a variety of motivational factors may account for the existence of the perseverative, stereotyped, inflexible, and invariable behavior which falls within the very generalized phenotypical concept of 'behavioral rigidity,' as, for example, fear of failure, uncertainty as to consequences, strong compulsive needs, inflexible goals, repeated failures, frustration, 'insecurity,' low success expectation, and others.

SATIATION AND CO-SATIATION

As an example of the author's construct of rigidity, one of the five experiments will be briefly reviewed. The general method was to state the hypothesis (that the degree of rigidity increases with age), to derive certain theorems, and to subject these to experimental test. The theorems were derived to provide conditions of testing in which the behaviors measured were consequences of the degree of rigidity and not of other factors that might produce 'behavioral rigidity.' The complete formalistic derivations of the five theorems tested by the author will not be repeated here, since they are available in the original study (1).

Three groups of subjects were used, all of approximately equal Binet mental ages: A group of Old Feeble-minded subjects with a mean chronological age of 41.7 years, mental age 80 months, and IQ of 42; a group of Young Feeble-minded subjects with a mean chronological age of 14.5 years, mental age of 82 months, and IQ of 48; and a group of Normal Children with a mean chronological age of 6.8 years, mental age of 82 months, and IQ of 99.8. All subjects were free from physiological or neurological pathologies as far as could be discovered by medical examination. On the basis of the theory that rigidity increases with chronological age it was predicted that, on all tests of rigidity employed, the Old Feeble-minded subjects would evidence the greatest degree of rigidity and the Normal subjects the least, with the Young Feeble-minded subjects showing a degree of rigidity greater than the Normal Children and less than the Old Feeble-minded.

One theorom was: "For a given mental age, the older an individual, the less effect a change of state of one region will have upon the state of neighboring regions." A particular instance of this general proposition was tested, namely, that "For a given mental age, the older an individual, the less will the satiation of one need co-satiate neighboring needs." This proposition follows from the general hypothesis that rigidity increases with chronological age. Since rigidity refers to the functional independence of neighboring regions it follows that, with increasing rigidity, a change of state of one region has decreasing effect upon neighboring regions. Satiation is one kind of change in the condition of a need region and co-

satiation is a measure of the effect of this change upon a neighboring need region or neighboring need regions.

This proposition was subjected to experimental test by satiating experimentally induced needs of the subjects and measuring the effect which the satiation of these needs had upon the state of neighboring needs (co-satiation). The subjects were introduced to four drawing activities on separate tables and actually drew samples of each (cats, bugs, turtles, rabbits). Following this they were allowed to draw cats until they were satiated with the activity. After satiation with drawing cats, they were allowed to draw bugs until satiated. Then they were allowed to draw turtles, and finally, rabbits. An index of co-satiation was computed for each subject, using the satiation time on the first drawing activity as the basis of comparison. On all three measures, and in line with the predictions made, there were significant differences in the degree of co-satiation manifested by the three groups of subjects; the Old Feeble-minded evidenced the least amount of co-satiation and the Normal Children evidenced the greatest degree of co-satiation.

With reference to the design of the experiment, effort was made to adhere to the conditions specified by the theorems. Assuming that mental age is roughly equivalent to the general degree of differentiation of the subjects, all subjects were of approximately equal mental ages. The relevant degree of differentiation was experimentally controlled. The subjects were familiarized with all of the activities *prior* to the measurement of satiation and co-satiation. There was a genuine reason for having the subjects actually draw each of the objects rather than merely showing them the drawing activities. Their drawing promoted successful experiences and thus reduced conditions that might produce 'behavioral rigidity' : 'insecurity,' fear of failure, uncertainty as to consequences, 'lack of confidence,' and so on. As an example of the absence of such facilitation, a subject who had not performed each of the drawing activities prior to the satiation of the cat-drawing activity might have been disinclined to engage in the other drawing tasks. This would give the false impression that the satiation of cat-drawing resulted in total co-satiation of the other activities. Actually this would be opposed to what was obtained and would be the consequence of inadequate attention to the conditions required and specified in logically derived propositions.

From Werner's discussion this author has been able to extract three objections to the experiments on co-satiation: (1) apparently contradictory results quoted by Lewin (4) from an unpublished study, (2) the 'structural' rather than 'functional' concept of rigidity, and (3) the possibility of alternative explanations.

The 'apparently opposite' results arrived at in the study quoted by Lewin were obtained by comparing feeble-minded children with normal children of the same chronological ages. The equivalence of age allowed no test of the hypothesis that rigidity is a function of chronological age. In addition, the groups were of different mental ages and varied in terms of the degree of differentiation. (In this connection, it may be well to indicate that Lewin referred to these groups as evidencing 'behavioral rigidity' and not dynamic rigidity as defined in terms of the degree of communication between neighboring regions. Lewin also explicitly stated that the results might be accounted for in terms of the degree of differentiation rather than by the construct of rigidity.) Of more importance is the design of the experiment. In the

experiment quoted by Lewin the subjects were satiated with drawing moon-faces after which they were asked whether they wanted to continue with free drawing. In the author's experiment the activities were preformed in that the subjects successfully engaged in each of the activities prior to the measurement of satiation and co-satiation in order to distinguish clearly between 'behavioral rigidity' and dynamic rigidity.

Let us consider Werner's second objection, namely, that the author's concept is 'structural' rather than 'functional' and fails to take into consideration the "dynamics underlying behavior." The author is uncertain as to what Werner means by 'structural,' and feels that his construct is 'functional.' The author also feels that he was considering the "dynamics underlying behavior" and that he further applied these dynamics by making them a part of the experimental design. By so doing, he was distinguishing markedly between rigidity as a quasi-descriptive generalization and rigidity as a construct from which strict derivations can be made that are capable of being subjected to experimental test. This avoids the pitfalls and apparent contradictions that might otherwise result by utilizing phenotypical, descriptive concepts.

Werner's third objection to the co-satiation experiments involves his providing an 'explanation' in terms of 'functional rigidity.' Werner's explanation follows: ". . . feebleminded persons are less easily satiated than normal individuals if the activity is monotonous or stereotyped; with spontaneous activity, normal subjects are less easily satiated than feebleminded subjects" (5, p. 45). Included in a preceding paragraph of Werner's are other explanations. "Any monotonous task, such as drawing moon faces or cats for a considerable time, is usually disagreeable to a normal child because it interferes with his desire for free and spontaneous activity. . . ." "As a rule feebleminded children are not opposed to monotonous work; they rather like it . . ." (p. 44).

It is difficult for the author to evaluate these 'explanations.' In the first place, the process of satiation demands monotony and repetition. There is no satiation without repetition inasmuch as these are the basic requirements for the process of satiation. To explain the experiment by saying that feebleminded children 'like' monotonous work more than normal children is hardly an explanation. As a matter of fact, if Werner's hypothesis is that the feebleminded children 'liked' the drawing activities better than did the normal children, one would have to predict that they would be more quickly satiated than the normal children, since the rate of satiation has been found to vary directly with the degree of centrality—the more liked an activity the more quickly is it satiated by repetition. The oldest subjects in the author's experiments had the longest satiation-times; the youngest, the shortest. In the second place, there was no evidence that the degree of attraction for the activities differed from group to group—the Old Feeble-minded subjects showed no more evidences of having a greater liking for the activity than did any other subjects as a group. The author agrees with Werner's generalization that "feebleminded persons are less easily satiated than normal individuals." That is, he agrees that this is an accurate summary of the author's findings in this particular experiment. The author would, however, differ from Werner in claiming that a repetition of the findings constitutes an explanation. The author would also disagree in using the fact that the subjects are feeble-minded as the primary explanation, since the

differences between the Old Feeble-minded subjects and the Young Feeble-minded subjects were as great as were the differences between the Feeble-minded as a single category and the Normal subjects. (On the basis of IQ there was no significant difference between the Old and Young Feeble-minded subjects.) It was this difference between the Old and Young Feeble-minded that constituted one aspect of the test for the author's hypothesis that rigidity is a function of chronological age. The relationship of rigidity to the degree of feeble-mindedness follows as a corollary proposition when mental age is equated.

The above comments about satiation time are not relevant to the author's hypothesis. The rate of satiation was not the issue, and no prediction was made as to the absolute satiation times. The theorem was in terms of co-satiation. The prediction following from the hypothesis that rigidity is a function of chronological age was stated as: "For a given mental age the less will the satiation of one region result in the co-satiation of neighboring regions." One might point out that this result obtained *in spite of* the longest absolute satiation times of the Old Feeble-minded subjects and the shortest absolute satiation times of the Normal subjects. Studies of mental fatigue, physical fatigue, and, more pertinently, satiation, have shown that the longer the time spent on an initial work or satiation period, the less is the time spent during following periods on similar activities. This finding about the relative degree of co-satiation cannot be logically derived from such statements as: "Feeble-minded children are less easily satiated than normal children," "everybody familiar with feeble-minded children has noticed the stereotypy even of their free activity," and, "as a rule feebleminded children are not opposed to monotonous work; they rather like it"; and similar descriptive generalizations stated in Werner's article comparing feeble-minded individuals as a class with normal children as a class.

In this connection, the author is inclined to point out another statement of Werner's. With reference to the experiment on co-satiation Werner says: "Kounin contends that easy shift from one (satiated) activity to another of the same sphere is a sign of rigidity" (5, p. 44). The author made no such statement. He was not measuring the 'easiness of shifting.' He was measuring the degree to which the satiation of one need co-satiated a neighboring need and correspondingly changed the valence of the satiated activities. No statement of 'ability to shift' as applied to this particular experiment was made, or intended, or could be logically derived from the postulates.

RIGIDITY AND DIFFERENTIATION

Werner makes considerable issue about the confusion between the concept of rigidity and the concept of the degree of differentiation. He starts his article by quoting what he considers to be evidence of disagreement between Lewin and the author:

> "Peculiarly enough, Kounin finds himself here in disagreement with the following view of Lewin: 'If our conception of a certain equivalence between higher degree of differentiation and external mobility be correct, then, under certain conditions, even with the feeble minded, a greater mobility of actual behavior should make itself noticeable with an increase in age'" (5, p. 43).

As the author elaborated in the statement of his original problem, he was not in disagreement with the above statement. The author has unequivocably stated that increasing degree of differentiation leads to increasing variability of behavior and external mobility.

Another quotation from Werner pertaining to the confusion between the author's concept of rigidity and degree of differentiation:

"The *surprising*[2] statement by Kounin, '. . . rigidity is a positive function of chronological age,' is obviously based on the *erroneous identification* of the two concepts. It is not difficult to see why a *structural* concept of rigidity might invite a *mistake* of this sort. Rigidity is here defined in terms of segregation of regions; the distinction between 'segregation of regions' and 'differentiation of regions' is easily *obscured*" (5, p. 46).

The author fails to see how this distinction is 'easily obscured.' The number of regions and the degree of functional segregation between particular regions are two distinct concepts.

Werner further quotes an experiment by Usnadze in which it was shown that, on various sorting tests, young children displayed little capacity to 'shift,' and, as they grew older, this capacity increased. He then states: "If Kounin were familiar with these well-known experiments, he hardly would have made the statement that rigidity increases with age" (p. 47). The author sees no point in disputing the assumption that Kounin was unfamiliar with the experiments of Usnadze or the evaluative statement that they are 'well-known.' The author does dispute Werner's conclusion as to what the author would have said. The author still proposes that rigidity increases with age. Usnadze's findings that as children develop they manifest more abilities with reference to card sorting is in no way contradictory to the author's hypothesis that the degree of rigidity increases with age.

The author does not desire to leave the summary comments following the preceeding paragraphs as authoritative pronouncements. He has stated that he agrees with theories that propose increasing capacities with age, increasing differentiation with age, increasing behavioral variability with age, and so on, at least as far as development from infancy to maturity is concerned. He has also proposed that the degree of rigidity, as he defined it, increases with age. He has also explicitly stated that behaviors sometimes called 'rigidity,' in the varied meanings of lack of variability, perseveration, stereotypy, external mobility, 'inability to shift,' and so on, cannot be used as direct measures of dynamic rigidity. He has explicitly stated his reasons for not using 'behavioral rigidity' as equivalent to rigidity and has specifically brought out that the degree of differentiation of a person influences the degree of variability, stereotypy, fixity, and so on, of his behavior. In other words, the author fails to see how he has 'confused' the degree of rigidity with the degree of differentiation. In the conceptual definition, the operational definitions, and the actual design and conduct of his experiments, he has made deliberate effort to deal with the degree of rigidity as distinct from the degree of differentiation. On the basis of this distinction, he equated not only the mental ages of all subjects but the degree of differentiation relevant to each experiment. He has even gone further by reducing the motivational factors (such as low success expectation, hesitance to enter unfamiliar regions, etc.) that might produce those varied types of behavior that are sometimes lumped together in the pseudo-descriptive category of 'behavioral rigidity.'

APPARENT CONTRADICTIONS

Werner mentions two other experiments of the author in an attempt to point out a 'contradiction.' The one pertains to a test of the theorem that, for a given degree of differentiation, the more rigid the individual, the less likely

[2] Italics the author's.

he is to be in an overlapping situation. The second pertains to a test of the theorem that the more rigid the person, the more difficulty he will have in the performance of a task which requires him to be influenced by more than one region. Both were predominantly motor performances. The first experiment was one in which the subject, in a series of trials, lowered a lever in order to release marbles. After ninety trials of lowering the lever, the subjects were instructed to raise the lever in order to release the marbles. As predicted, the Old Feeble-minded subjects made the fewest number of errors (fewer lowerings of the lever after having been placed in the situation of raising the lever); the Normal subjects, the greatest number; and the Young Feebleminded subjects, an intermediate number of errors. In the second experiment the difficulty (time and errors) of performing card-sorting tasks in simple and in overlapping situations was compared for each subject. As predicted, the Old Feeble-minded subjects evidenced greatest difficulty in sorting cards in an overlapping situation relative to their performance in sorting cards in non-overlapping situations; the Young Feeble-minded subjects evidenced the least relative difficulty performing the task in an overlapping situation. The theorems, for which these experiments were devised as tests, were logically derived, and the predictions of the results were made prior to the conduct of the experiments.

Werner labels the results as 'contradictory.' He claims:

"It seems hardly possible to reconcile this contradiction on the basis of a structural concept of rigidity. On the motor test,[3] feebleminded persons acted closest to normal adult behavior, whereas in the card-sorting test, the young normal children did.[4] Therefore, logically, the results of either one of the two tests, but not of both, can prove a high degree of rigidity of feebleminded persons. The truth most probably is that the feebleminded behave more rigidly on the cardsorting test, but less rigidly on the motor test, than do the normal children" (p. 47).

Actual events are neither logical nor illogical. Inability to apply the same label to selected behaviors occurring in different situations does not make the experimental results 'logically contradictory.' To the author the results are not contradictory, having been logically derived. The author deliberately chose two motor performances to increase the degree of validity of his hypothesis, and pointed out that the power to predict otherwise seemingly contradictory results was one of the advantages of the construct of rigidity.

The author is inclined to believe that a phenotypical concept of rigidity is what brings Werner to the opinion that the results are contradictory. Thus, the results appear to Werner to be contradictory because the feeble-minded children acted "closest to normal adult behavior" under one set of conditions whereas the young normal children acted "closest to normal adult behavior" in another. Werner states: "The truth most probably is[5] . . . that the feebleminded *behave*[6] more rigidly on the

[3] Werner is here referring to the test involving lowering and raising of a lever. (The card-sorting experiment was also a 'motor' performance.)

[4] A factual comment is here warranted. The Young Normal children, for reasons stated in the original study, were not utilized in the card-sorting experiment. Throughout his discussion, Werner continues to neglect the crucial matter that the author is not using feeblemindedness as a homogeneous category. The differences, in all experiments, between the Old and Young Feeble-minded (of equal mental ages and practically equal IQ's) were sizeable and significant and were predicted on the basis of the theory of rigidity changes with chronological age.

[5] The actual results are published.

[6] Italics the author's.

card-sorting test, but less rigidly on the motor test . . ." (p. 47).

The author is unclear as to the referent for the phrase "behave more rigidly." What is meant: make fewer mistakes, require more time, "act less like normal adults"? The author is obliged to repeat that he has not defined rigidity as any 'type of behavior,' or as any constant behavioral 'trait.' He defined rigidity as a construct with only one meaning. The operational definitions (behaviors) were specifically stated in relation to the nature of the conditions and not in terms of any unchangeable action obtaining in any kind of situation and under any state of the individual. Nowhere has the author defined rigidity as "mistake making," "time consuming," "behaving unlike normal adults," or any other type of action, as such.

In connection with the above experiments, another statement of Werner's, made in a different part of the article, calls for an answer. He states: "The inadequacy of a structural concept of rigidity can be further shown by the fact that 'overlapping' and not segregation frequently leads to perseveration. . . ."

The author, to the extent of devoting an experiment to the proposition, has explicitly stated the conditions in which overlapping does lead to 'perseveration.' The experiment involving raising and lowering of a lever was designed as a test for the theorem that the person with the greatest degree of rigidity is least likely to be in an overlapping situation. The prediction was that the Old Feeble-minded subjects would make the fewest perseverative errors (as defined in that particular experiment by lowering the lever after being instructed to raise the lever); that the Normal subjects would make the greatest number; and, that the Young Feeble-minded subjects would make an intermediate number of perseverative errors.

RIGIDITY AND BRAIN INJURY

Werner makes several statements about what the author would have said concerning brain-injured children. For example, he states: "Employing the structural conception of Kounin, one would be led to the absurd conclusion that brain-injured children are less rigid than normal children and non-brain-injured feebleminded subjects" (p. 45).

Whether 'absurd' or not, the author made no statements whatsoever about brain injury in relation to rigidity. In all publications pertaining to his studies on rigidity the author made it clear that subjects with any history or signs of physiological or neurological pathologies were excluded from the experiments. The author feels unable to hypothesize, with any significant degree of confidence, any correlate between rigidity as a psychological construct and its equivalent in a neurological theory. Whether this may eventually be accomplished is another matter. With respect to the quotation in the preceding paragraph, as well as to other statements of Werner's as to what Kounin would say about brain injury in relation to rigidity, it is essential to reiterate that Werner's concept of rigidity differs from the author's. His attribution of conclusions to the author generally are not conclusions consistent with the author's, nor implications of the author's definitions and postulates, even though the same term (rigidity) is used by both.

GENERAL COMMENTS

This article is not intended as an evaluation of Werner's concept of rigidity. It contains no reference to any of Werner's contributions to the field. It is restricted to the one article of Werner's. Moreover, in respect to this one article, the author has attempted to limit his discussion to those sections dealing with this author's formulations

and Werner's statements about this author's formulations. While the author doubtlessly has his own opinions about the matter, he intends no conclusions about whether he or Werner is 'correct,' or about whose concept of rigidity is the more scientific, logical, or productive. The author's reactions to the tone of Werner's 'Critical Evaluation' is such that he considers this paper as little more than a recapitulation of some of his own concepts in order to avoid confusion among those who may have been led by Werner's article to misinterpretations of this author's study.

The author cannot help but react to the nature of Werner's 'Critical Evaluation.' It seems to him that it contained too many emotionally toned labels and authoritative pronouncements of opinions and conclusions without adequate and explicitly stated reasons for the conclusions. Misunderstandings were especially likely to result from such a procedure when Werner undertook to make statements about what this author *would* have said about certain matters (as, for example, Werner's comments about brain-injured children).

Examples of unelaborated labels found in Werner's article are: 'structural,' 'erroneous,' 'mistake,' and 'absurd.' Authoritative pronouncements of opinions and conclusions are exemplified in the following quotations: ". . . it [this author's definition] does not take into consideration the functional dynamics underlying behavior"; "The usefulness of this novel definition appears to be doubtful"; and results are 'illogical' and 'contradictory.' The last few opinions are interesting to note because they point to omissions which the author considers important. The author devoted an article (3) exclusively to the discussion of the fruitfulness of the construct of rigidity. In this article he enumerated criteria for fruitfulness and illustrated their application to the construct of rigidity. Werner failed to mention this article or suggest any concrete criteria for fruitfulness; he dispensed with the problem by simply stating that "the usefulness of this novel definition appears to be doubtful." As to 'contradictory' and 'illogical' findings, the author wishes to point out that in his complete manuscript (1), which Werner also failed to mention or include in his references, he explicitly stated the assumptions, postulates, and steps of logic utilized in arriving at the theorems. The author, not being a professional logician, does not doubt that some of his formulations are subject to logical inadequacies, nor does he claim that his explanations are final. He does not feel enlightened, however, by Werner's merely calling results 'illogical' or 'contradictory.'

The author feels that the differences between Werner and himself in respect of the concept of rigidity are not as great as might seem if the assumptions and definitions were explicitly and unequivocably stated. He does not feel, however, that communication will be achieved by indiscriminate use of labels or that intelligent revision will be arrived at as a result of authoritative pronouncements of opinion.

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SOME HYPOTHESES FOR THE ANALYSIS OF QUALITATIVE VARIABLES

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I. Introduction

The advancement of psychology as a science rests primarily upon the advancement of measurement theory. New advances, like all inventions, are fundamentally a repatterning of existing concepts; old ideas are put together in a new perspective and new concepts emerge. These new concepts are then buffeted about on paper and in the laboratory. Some, perhaps most, get knocked out of the ring and the rest take their place among the accepted concepts in scientific psychology. Examples of the latter are the foundation stones (too numerous to list) laid by Thurstone in the fields of psychophysics and factor analysis. Guttman (3, 4) has extended these basic contributions in some papers on the theory and technique of scaling qualitative data, and Festinger (2) has recently published a review of the literature in this field.

The concept of a scale has been made clear by Thurstone's (6) development of the law of comparative judgments. In his review Festinger (2) illustrates the concept in detail and the manner in which it is applied by Guttman and his co-workers. The contributions of Guttman have all been pointed in the direction of how to purify a set of items in a questionnaire in order to select that subset of items the responses to which are generated by the same function. This, unfortunately, is a rare event in the domain of psychological traits. There is a tendency in Guttman's papers to the effect that an instrument is not worthwhile if the above criterion is not met. In the opinion of the writer this is a serious error. We will point out in this paper that the responses of a group of individuals to an instrument, be it a personality questionnaire, attitude scale, or paper and pencil test, potentially contain the essential elements for six separate scores. Each of these scores has its particular significance. Psychological measurement theory already contains the basic concepts necessary to the proper design of instruments to permit the determination of these six scores and their significance. One of these scores, here called the trait status score, is indicative of the degree to which the trait or variable measured by the instrument is generated by a unified function. Hence, the trait status score is an index of the degree to which the performance of a sample of individuals on an instrument has met Guttman's concept of a perfect scale.

The general procedure in searching for scales is to administer a questionnaire containing items believed, on more or less a priori grounds, to belong to or to measure the same domain. The responses of the subjects to these items are then analyzed to see if, among this sample of items, there is a subsample which forms a scale. If a scale is found, all the remaining items are then thrown out and the scale items only are used. It is the writer's conviction that much of the potential value of the instrument is lost.

One of the dangers of this procedure

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lies in the likelihood that the domain defined by the subsample of scalable items is a restricted partition of the domain originally intended to be measured. For example, a questionnaire composed of items designed to measure attitude toward the British may, with one population sample, yield a scale only among those items that measure attitude toward British foreign policy. In another instance, a collection of behavior items belonging to the domain of honesty may, in that population sample, yield a scale only among those items pertaining to lying or stealing.

Another consequence of selecting only scalable items is the restriction, if not the almost complete destruction, of the potential clinical value of the questionnaire for the analysis of an individual. Guttman appears to recognize the clinical significance of deviants from a scale when he says, "As a matter of fact a study of the deviants is an interesting by-product of the scale analysis. Scale analysis actually picks out individuals for case studies" (4, p. 149). And yet by restricting his final measuring instruments to the truly scalable items he has eliminated the possibility of deviants among individuals who are from the same universe as the original sample. To the degree to which the instrument is confined to scalable items its potential clinical significance is also confined.

II. THE GENESIS OF PSYCHOLOGICAL TRAITS

To make these matters clearer let us digress and consider what gives rise to a scale in the first place. In the infant the psychological domains of mental abilities, personality traits, character, and attitudes are either nonexistent or a relatively undifferentiated mass of potentialities. As the child approaches maturity some of these potentialities come to be realized. Separate mental abilities, various personality traits, and a variety of attitudes differentially develop. One might say that these domains become structured. The degree to which this organization or structuring arises through the mutual influence of maturation and environment is not relevant to our problem. Probably both types of influences enter to different degrees into the organization of different domains and even within a given domain. In the domain of personality, for example, some traits may be more the result of biological differences and others more the result of cultural or educational differences. The important thing for the understanding of scales is that organization and structure appear as unified psychological traits in these various domains. Some traits may be found to be common to various cultures and some peculiar to a particular cultural framework.

Because of the variety of influences which generate them, psychological traits are unique to some extent in every individual. But in a highly organized social order with standardized education there will tend to be certain traits generated which will be common to the population subjected to the same pattern of forces. There is, however, at the same time, opposition, contradiction, and interaction of these forces on organisms that are not equally endowed in the first place—with the result that the structuring of a psychological trait is less complete in some individuals than in others. The degree to which this structuring has occurred in a particular individual with respect to a particular psychological trait should be of considerable clinical significance.

A psychological trait, in other words, may or may not be a functional unity and it may or may not be general, *i.e.*, common to a large number of individuals. A psychological trait is a collection of related behavior elements in which a psychologist is interested.

These elements may be integrated or organized to a varying extent in different individuals. The more completely integrated the elements are in an individual the greater is the dynamic functional force of the trait. For example, a collection of items in an arithmetic test defines a psychological trait. Unpublished studies of such tests by scale analysis have indicated the existence of remarkably good scales. This is to be understood in terms of the high standardization of educational procedures and progression of training which give rise to the functional unity we call arithmetic ability.

On the other hand, a psychologist may be interested in the state of morale of a group of workers in a company. He assembles a collection of items sampling specific attitudes toward many phases of the company's policies and operation. The questionnaire might contain items sampling the employees' attitudes toward hours of work, environmental conditions, wages, personnel policies, etc. These items may or may not be related to each other, that is, comprise a scale. The morale of an individual worker may be regarded as a distillation or resolution of a heterogeneous assembly of a number of these lesser attitudes toward the company. An individual worker is regarded as having high morale if he indicates a favorable attitude toward the company on a high proportion of specific attitudes, and as having low morale if he has a high proportion of unfavorable attitudes.

It may be noted here in passing that the writer is in complete agreement with Conrad (1) with respect to his position on uni-dimensional scales. It is highly desirable to search for such instruments, but sometimes we appear to act as if uni-dimensionality were a property of the instrument instead of a property of a group of individuals. For one group of individuals the collection of items in a questionnaire may represent a cohesive series of steps in a psychological continuum. For the members of this group, that collection of items represents a psychological trait which is common to the group and integrated into a functional unity. This situation is rarely found in practice; what is needed, in part, is an index of the extent to which the scale exists for an individual, for a group of individuals, and for the trait itself. We shall propose certain indices which may be so interpreted.

III. THE STATUS SCORES

We are applying the term 'status' to the score obtained in the conventional manner on a test of mental ability, a neurotic inventory, or other instrument. This is done, for the purposes of this paper, to prevent confusion with several other kinds of scores which may be obtained from the same instrument at the same time, because each of these scores is subject to different interpretations. The status scores are conventionally interpreted as the relative position of individuals with respect to what is being measured by the instrument. A status score may be interpreted roughly as the position of an individual relative to other individuals.

There is a status score for each individual and a status score for a group of individuals as a whole—the latter being merely the average of the status scores of the individuals comprising the group.

IV. THE DISPERSION SCORES

The status score theoretically represents the amount of some trait or ability which an individual possesses. Hence, if a trait were perfectly integrated and organized, the individual would pass correctly every item sampling a level of ability below his and fail every item at a level of ability above his. But because psychological traits are not usu-

ally so well structured, individuals' performances are inconsistent. An individual may fail some items which are below his ability as determined by the performance of a group of individuals and may pass some items which are above his ability. The score which represents the degree of this inconsistency we call the 'dispersion' score. This score may be obtained by computing the mean deviation or standard deviation or some other measure of variability of the inconsistent responses.

The dispersion score, consequently, is a measure of the degree of structuring or integration of the trait within the individual. This score is significant of something rather different from the status score but perhaps nonlinearly related. This is a matter for experimental study.

Thurstone (7) in one of his early articles on the measurement of attitudes suggests that the dispersion of the items indorsed by an individual is an index of his tolerance on the issue measured by the attitude scale. This hypothesis has been undeservedly neglected in later experimental studies.

Let us return to the example of the morale questionnaire. The items of the questionnaire could be ordered on the basis of the proportions of the population indicating favorableness or unfavorableness on each. Then for each individual there would be two scores; one would be the proportion of items in which he indicates a favorable attitude, representing his status score, and the other would be his dispersion score. The latter score would be determined by the individual's unfavorable responses on items to which he could be expected to respond favorably (in view of his status score), and also by his favorable responses on items to which he could be expected to respond unfavorably. The significance of a dispersion score would be different for different psychological traits and has to be investigated separately for each. In the case of the psychological trait of morale, the writer would be inclined to test the hypothesis that the dispersion score of an individual would be indicative of the rigidity or susceptibility of the morale of this worker to change—the degree to which his morale is a functional unity—a dynamic psychological trait.

The potential clinical significance of this concept of scales might be made clear by a further example. A personality trait is created in an individual as a result of a variety of forces over a period of time. The extent to which a particular personality trait is formed, such as honesty for example, will vary among individuals. A collection of behavioral items pertaining to honesty could be ordered in terms of the prevailing standards of society. An individual's over-all degree of honesty would be reflected by the proportion of situations in which he would behave honestly. Two individuals may have the same honesty (status) scores, and yet differ widely in their dispersion on the ordering of these situations as determined by society. The individual with very small dispersion has accepted the precepts of society and has an integrated trait of honesty to the degree indicated by his dispersion score. The individual with wide dispersion is in conflict with society's values in this domain or has not formulated a generalized principle of honesty.

The dispersion score of a group as distinguished from that of an individual is determined by the *dispersion of the status scores* of the individuals comprising the group. A high dispersion score signifies that the members of the group do not have similar positions on a scale but are diversified. A low dispersion score signifies that the members of a group all have similar positions on the scale, that the group is unified. It

would be indicative, for example, of the extent to which the group could be expected to exhibit a united front in the domain tapped by the instrument. It is the group dispersion score that Conrad (1, p. 587) is speaking of when he says, "Our purpose was to discover whether the sample's current attitudes were homogeneous and unified, or heterogeneous and divergent. For this purpose we are inclined to agree with McNemar's (5, p. 328) statement, . . . that 'variation within groups indicates the relative homogeneity of groups in their opinion about an issue.'"

V. THE TRAIT STATUS SCORE

The 'trait status' score is in some ways one of the most important scores of all. It is determined by the *average of the dispersion scores* of the individuals comprising the group. A low trait status score indicates that the trait in question exists as a functional unity, that it is a *common* psychological trait within that group. A high trait status score indicates that the function being measured is not an organized integrated trait, but rather that the elements in the instrument are a heterogeneous unrelated collection. The trait status score is directly related to the degree to which a scale exists. If a scale exists the trait status score is low, *i.e.*, the respective members of a group have low dispersion scores.

Let us return to the morale questionnaire and assume it was administered to the workers in two separate plants. The interrelation of the specific attitudes contained in the questionnaire may be high in one plant and low in another. In that plant in which the trait status score reveals the existence of a scale running through the elements the morale of the workers is a highly structured functional unity. The experimenter would be motivated to see what in the situation was bringing about this 'halo' effect, what had created this generalized attitude which was being reflected in all the elements. The low trait status score might also indicate that the attitudes or morale of the group would be less subject to change under the influences of propaganda. There might even be the basis here for experimentally determining a rational origin for 'indifference' in the morale scale. If a scale exists in the items measuring morale, those individuals on the favorable side of morale would be more affected by favorable propaganda than unfavorable. Similarly those individuals on the unfavorable side of the morale scale would be more influenced by unfavorable propaganda. This effect may make itself apparent in a shift of the status score of the individual or a contraction of his dispersion. In a plant in which no scale is found in the morale elements comprising the questionnaire, two workers may have the same score or proportion of favorable attitudes but have very different specific attitudes. If a scale were not found to exist in the morale questionnaire for this group of workers, it would indicate the lack of any organizing or structuring force, and hence that the workers might be more susceptible to either favorable or unfavorable influences.

In a recent paper Thurstone (8) emphasizes the potentialities of the discriminal dispersion in the prediction of conduct. He says, "In the measurement of social attitudes of a group it is not only the average affective value of a proposal or idea that is of significance but also the dispersion of affective values within the group. It may even be possible to *define the morale of a group in terms of the sum of affective dispersions of all its debatable issues.* . . . Measurement of the seriousness of crimes can be made by psychophysical methods in which the dispersions are

signs of heterogeneity or lack of unity in the group and its code" (8, p. 248).

This is almost identically the thesis of this paper except that where Thurstone is talking about the discriminal dispersion of items on the scale, we are talking of the dispersion of the items for an individual and the dispersion among individuals. We would define the morale of a group as the status score of the group. But in predicting the behavior of the group in terms of the likelihood of a strike, for example, the group dispersion score would be very significant. Other things being equal, the greater the group dispersion score the less the likelihood of any concerted action by the group. In other words, the group status score would be indicative of the direction the group would take if it took action and the group dispersion score would be indicative of the likelihood of any action being taken. We would hypothesize further that the trait status score for this same group would be indicative of the amount of effort that would need to be expended to change the direction of the group's action. A low trait status score would indicate that the individual members hold firm convictions, that their morale, whether it be good or bad, is a highly structured functional unity. Their morale is not as susceptible to change as that of a group of individuals whose average dispersion score (trait status score) is greater.

The trait status score, representing the degree of integration or scalability, will differ among the infinity of psychological traits, just as, for a given trait, individuals will vary in their degree of integration as represented by their dispersion scores. From the point of view of factor analysis, the fact that a scale does not exist in a collection of items means that the total score is a function of more than one factor, it is not a unitary trait. But the fact that a scale does exist in the collection of items and reveals a unitary factor in a factor analysis of the items, does not necessarily mean that the psychological variable being measured is primary. What it could mean is that the pattern of primary factor loadings of all items is the same.

It is this fact that adds to the confusion on the subject of uni-dimensionality. It is possible for a psychological trait to act as an integrated functional unity (low trait status score—uni-dimensional) and yet be a function of a number of independent or related factors. This would only be revealed if the battery being factored included relatively pure measures of the factors. For example, the score of an individual i on item q is defined as follows:

$$s_{iq} = \alpha_{q1}x_{1i} + \alpha_{q2}x_{2i} + \ldots + \alpha_{qr}x_{ri},$$

where the α's are the loadings of the item q on each of the r factors, and the x's are the loadings of the individual in each of the respective r factors.

The individual's score on a trait t may be defined factorially as

$$S_{it} = \beta_{t1}x_{1i} + \beta_{t2}x_{2i} + \ldots + \beta_{tr}x_{ri},$$

where the β's are the loadings of the test on each of the r factors.

The condition that a psychological trait be perfectly integrated into a functional unity is the condition that

$$\alpha_{11} = \alpha_{21} = \ldots = \alpha_{q1} = \ldots = \beta_{t1}$$

$$\alpha_{12} = \alpha_{22} = \ldots = \alpha_{q2} = \ldots = \beta_{t2}$$

$$\vdots$$

$$\alpha_{1r} = \alpha_{2r} = \ldots = \alpha_{qr} = \ldots = \beta_{tr}$$

This condition, of course, would rarely, if ever, be met. The degree to which this condition is met, however, would signify the degree to which a functional field is integrated.

VI. THE TRAIT DISPERSION SCORE

The 'trait dispersion' score is defined as the dispersion of the individuals' dispersion scores. The significance and interpretation to be given this score is more speculative than any of the others and, like the others, should be subjected to intensive experimental validation. In line with the hypotheses already presented, however, it may be suggested that this score may be indicative of heterogeneity of the strength of forces bringing about integration of a psychological trait. The dispersion score of an individual, it will be recalled, is interpreted as the degree to which the psychological trait in question is integrated into a functional unity in the individual. If the trait dispersion score is high, this would indicate wide variability among individuals in the degree to which this has occurred. One of the possible reasons for this would be the lack of standardized forces influencing the integration of the trait from individual to individual.

VII. CONCLUSIONS

There are inherent, then, in any instrument designed to measure a psychological trait, six separate scores. Hypotheses are presented as to the significance of each of these scores for different aspects of the trait.

1. There is the individual's 'status' score which is the conventional score in an objective test of mental ability or the number of favorable responses in an attitude questionnaire or similar instrument.

2. There is the 'status' score of the group which is simply an *average of the individuals' status scores*. This represents the average ability, average attitude, or average value of the group taken as a whole with respect to the trait in question.

3. There is the individual's 'dispersion' score which is a measure of his dispersion over the items and is indicative of the extent to which the trait being measured is a functional unity for *him*. Because the absolute magnitude of this score would be a function of essentially irrelevant matters—such as the number of items in the scale and their distribution of scale values—it perhaps should be expressed as a ratio of the 'trait status' score below. This would indicate the extent to which the trait was a functional unity for the individual as compared with the average individual of the group.

4. There is the 'dispersion' score of the group which is the *dispersion of the individuals' status scores*. This represents the cohesion or uniformity or rigidity of the group—the similarity of the individuals in the group with respect to the trait in question.

5. There is the 'trait status' score of the group which is the *average of the individuals' dispersion scores*. This represents the extent to which the trait in question exists as a functional unity in the group. It is only if this value is sufficiently low that the instrument may be said to measure a unitary trait. This is, in effect, the score that Guttman tries to make as low as possible by scale analysis. It is desirable that psychologists isolate these traits in the domain of personality and develop measuring instruments for them, for these are general personality traits. As has been pointed out, however, an instrument is not worthless because the trait status score does not indicate the trait to be a general functional unity. So long as the items meet some suitable criterion for inclusion in the domain, the other four scores are still significant in their own right and for their special purposes.

6. There is the 'trait dispersion' score which is the dispersion of the individuals' dispersion scores. This score may be interpreted as indicative of the heterogeneity of the strength of forces from

one individual to another bringing about integration of a psychological trait.

VIII. SUMMARY

Potentially, then, any mental ability test, personality questionnaire, neurotic or adjustment inventory, or attitude questionnaire could be made to yield six scores. Two of these are the conventional scores usually obtained—the individual's status score and the group's status score. The other four scores are derivatives of the dispersion within the individual and between individuals. They are indices of the extent to which a trait exists as a functional unity in general (the trait status score); the extent to which there is integration (rigidity, conformity or maturity) of the trait in the individual (the individual's dispersion score); the extent to which there is organization, cohesion, or uniformity within the group (the group dispersion score); and the degree to which there is uniformity or heterogeneity in the strength of the forces bringing about integration of a trait from individual to individual (the trait dispersion score). The particular significance or interpretation of these scores is dependent upon the nature of the psychological trait in question. The explanation of integration or lack of it may be in terms of maturity, conflict or uniformity of social forces, standardization of education, or other dynamics.

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BEHAVIOR THEORY AND THE BEHAVIOR OF ATTITUDES: SOME CRITICAL COMMENTS

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I. Introduction

In his paper on 'The Behavior of Attitudes' (3) Professor Doob has contributed a lucid and thorough analysis of the concept of *attitude,* one which will no doubt have considerable impact on social psychologists and on the status of social psychology. It is desirable to examine his argument carefully and to encourage the fullest discussion of it because, as will be explained shortly, it constitutes an interesting test of behavior theory and because, if his conclusions are accepted, they may have serious implications for social psychology.

In brief, Professor Doob examines attitudes in the light of behavior theory [1] (the theory of behavior developed by Hull and his associates) and comes to the conclusion that it would be tempting to propose (if such a proposal had any prospects of success) that the concept of attitude be dropped from social science, that the concept serves a quasi-scientific need, and that when the concept will finally be abandoned, it "will be a happy day for social science, since this event will signify the emergence of a more integrated and scientific system of human behavior" (p. 155).[2, 3]

If Doob takes his conclusions seriously, he should himself anticipate the future by dropping the word 'attitude' from his own vocabulary and use his not inconsiderable influence to persuade others to do likewise. It is perfectly true that, as Doob (p. 155) says, one cannot legislate popular quasi-scientific terms out of existence; but, then, neither is one compelled to compromise

[1] We may perhaps register a mild protest at the appropriation of 'behavior theory' for a particular theory of behavior, but we shall retain this usage in the present paper.

[2] All page citations refer to the paper under discussion (3).

[3] It should be noted that Doob's main dissatisfaction with the concept of attitude is not on the ground of its vagueness and lack of precise definition. This type of criticism of many psychological concepts occurs with sufficient frequency, however, to justify some comment on it. The present writer, at least, does not consider the demonstration that a term is ambiguously and inconsistently used as sufficient reason for discrediting it in scientific discourse. Words that have real referents are necessarily as ambiguous as their referents are vaguely apprehended. To abandon a word because it is not precisely defined is to give up the scientific quest before it has begun. One begins by vaguely observing something (whether it be *matter,* or *intelligence,* or *attitude*) and gradually sharpens one's observation until the word designating this observed something is more and more precisely defined. Science, after all, begins in ignorance.

Sometimes the process of sharpened observation leads to the discovery that the word designates a null class, that it has no real referent, and that the original vague observation was illusory. This Doob does not attempt to show with regard to *attitude.* Sometimes the process of sharpened observation leads to the discovery that the word designates a class of referents which are included in the referents of another word and which are not distinguishable from the latter. Such a discovery leads to greater unification of science, and this Doob attempts to do with regard to *attitude.* Such attempts should, however, be regarded with great caution. To be prematurely carried away by them is to give up the investigation of a separate and possibly real phenomenon.

It follows from our conception of the evolution of scientific definitions that an argument with regard to a definition is not merely a quibble over words; not if the definition is to be regarded (as it should be) as the best possible summary, in the light of available knowledge, of the essential nature of a real referent.

with quasi-scientific popular trends, especially in the scientific literature. No one legislated the word 'soul' out of existence; more and more psychologists simply stopped using it. That Doob, who sees the light, does not choose to lead the way suggests that he himself is not completely convinced by his own argument.

If he is right, then whether he is willing to lead the way or not, other psychologists should stop talking about attitude. It also follows, if he is right, that they should stop wasting their time in trying to investigate attitudes, as such. To say that the concept is 'quasi-scientific' is to say that its referent is falsely distinguished. Such a conclusion would, of course, be of profound significance to social psychology so much of which is devoted to the study of attitudes.

Early in his discussion Doob raises "the question as to why a simple, common-sense, ubiquitous concept like attitude should be translated into semi-technical jargon" (p. 135). He offers two answers. First, he points out that it is "desirable to bring as many terms as possible relating to a field of research . . . within one universe of discourse" (p. 135). Second, and what he regards as more important, he argues that "a tentative translation of a term from one level to another and into an already and perhaps better developed theoretical system is fully justified if thereby inaccuracies on the higher level can be pointed out" (p. 135).

Doob's intention is very laudable, but it is well to remember that there are certain dangers in such an attempt to achieve greater unity of science. In the first place, one must beware lest the attempted conceptual translation become a disguised conceptual substitution. In other words, one needs some assurance that the translation still involves the same referents. Insofar as substitution is permissible, it is only in the direction of conceptual refinement; and it must be shown that what is excluded really does not belong with what is included and that new inclusions do belong. This must, of course, be done on the level of the referents (observation) rather than theory.

From this point of view the attempted unification is as much a test of the "already and perhaps better developed theoretical system" as it is of the concept which is being reduced to this system. If, in translation, the concept is found wanting, the system rather than the concept may be at fault. Without examining behavior theory itself in detail, therefore, one can to some extent test its adequacy by determining how good an account it gives of phenomena such as attitudes. And if there are inadequacies in Doob's treatment of attitude, their examination and correction is as important to behavior theory as it is to the many psychologists who are as yet not convinced of the soundness of behavior theory.

II. THE NATURE OF *ATTITUDE*

Doob defines an attitude (p. 136) as "(1) an implicit response (2) which is both (a) anticipatory and (b) mediating in reference to patterns of overt responses, (3) which is evoked (a) by a variety of stimulus patterns (b) as a result of previous learning or of gradients of generalization and discrimination, (4) which is itself cue- and drive-producing, (5) and which is considered socially significant in the individual's society."

The bulk of Doob's paper is devoted to the explanation and amplification of this definition and of the related points of behavior theory. Some highlights of the definition are, in the light of Doob's discussion, as follows: An attitude is a learned response; it may be evoked by any one of a variety of stimulus pat-

terns; the latter need not have been directly associated with the response, but may through a process of secondary conditioning have become associated with a stimulus which has been directly associated with the response, or they may be sufficiently similar to the latter stimulus to evoke the same response; an attitude is itself not merely a *response,* but also a *stimulus* which may evoke any one of a variety of learned responses; it is, moreover, the kind of stimulus which is tension producing and, consequently, some of the learned responses to it (*i.e.,* goal responses) have the effect of reducing the tension (reward); it cannot itself be observed by an outsider, but the responses to which it gives rise are observable; it is no different from any other learned, implicit, cue- and drive-producing response such as a rat's aversion to a grid on which it has been shocked; but it may be arbitrarily distinguished from other such responses on non-scientific grounds (social significance). Other points of Doob's definition and elaboration will also be commented on in the following discussion.

By way of contrast and to serve as background in our discussion of Doob's concept, we present an alternative conception of the nature of attitude, an alternative which we prefer and which we believe will be a recognizable version of the common usage of the term.

An attitude, according to this conception, is *a disposition to evaluate certain objects, actions, and situations in certain ways.* For purposes of the present definition, evaluation requires words; the *disposition* to evaluate need not, however, be verbalized. An attitude may be conscious (recognized and known by its 'possessor') or it may be unconscious. It may be momentary or persistent. If it persists, it becomes salient in situations to which it is pertinent; otherwise it may be relatively unobservable, except through its effects. It may pertain to matters socially significant or insignificant. The evaluations involved may correspond to, or differ from, the evaluations of most members of the culture or subculture or the 'coded' cultural and institutional values. They may or may not have strong personal pertinence, self-reference, or ego-involvement. They may or may not be associated with strong impelling motives. The disposition to evaluate in certain ways is generated by such factors as one's available knowledge concerning the object, action, or situation, one's comprehension of the interrelatedness and interrelations of phenomena and of the consequences of actions, one's time perspective, one's motives and motivational displacements or substitutions, one's conscious and unconscious patterns of phenomenal equivalences (symbolisms in the psychoanalytic sense), one's introjected moral values, and the interaction of these factors in relation to other attitudes. Various aspects of this conception will also be elaborated upon in the course of the following discussion.

From the viewpoint of this second conception of the nature of attitude, when Doob writes (p. 145) that "The rat's aversion to a grid on which it has been shocked is doubtless implicit, it is anticipatory and mediating in avoiding the shock, it can be evoked by a gradient of stimuli, and it has drive value resulting in other responses which serve perhaps to allay its anxiety; but it is ordinarily not useful to call such a response an attitude," we may add that not only would it be not useful to call such a response (or response pattern) an attitude, but that it would be wrong. Rats may have aversions, but they do not have dispositions to evaluate which, in this case, would require a disposition to verbalize the aversion.

It should be noted that, although the

two conceptions are assuredly different, there are many points of agreement or near agreement. Both would, for example, agree that (1) a person is not born with his attitudes, (2) the learning process plays a major role in the development of attitudes, (3) attitudes involve problems of perception and motivation, (4) as a result of a particular attitude a person may be more likely to perceive certain objects than others, (5) some attitudes affect perception after their arousal even though they may not have oriented the person originally in the direction of the perceived objects, (6) specific behavior cannot be safely predicted from a knowledge of attitude alone, and (7) people may act contrary to their attitudes.

For reasons of convenience, we shall present our subsequent discussion of Doob's analysis of the concept of attitude in several sections. First, we shall concern ourselves with some points of Doob's analysis concerning which it seems to us that he is inconsistent. Second, we shall deal with several debatable points which are not required by behavior theory; such points may, of course, be amended without challenging the viewpoint or possible validity of behavior theory. Third, we shall concern ourselves with several points which seem to be crucial to behavior theory. We recognize that this is not a 'clean' division. The first category, for example, must overlap with the second and third; but, until the apparent inconsistencies are clarified, it would be premature to place the involved points in the second or third category. There may also be some dispute over the placement of specific points. In spite of our concentration on Doob's analysis, however, our major concern is with the clarification of the concept of attitude rather than with the soundness of his position. If it had no other merit (and, of course, it does), Doob's analysis highlights some aspects of attitude which are not too frequently discussed.

III. Points of Inconsistency in Doob's Definition

We may begin by remarking on a basic inconsistency in Doob's approach, his confessed stepping beyond what he regards as the bounds of science in order to define attitude. If it is true, as Doob claims, that the social significance of a phenomenon does not belong "in a set of systematic theories which together approximate something that might be called science" (p. 155)—a point we shall later dispute—, then Doob had the alternative of omitting the last point of his definition and asserting in its place that some 'attitudes' attract special attention because of their social significance; and that the students of attitude should not be misled by the social significance of some into ignoring the many others which are not socially but are scientifically very significant. This would have had the effect of unifying the phenomena which Doob believes really belong together, but not the effect of ruling *attitude* out of the realm of sound scientific discourse. Of course, with no *differentium* at all provided, the reader may have been forcefully reminded of a number of other psychological concepts which have suffered the same fate at the hands of behavior theorists (*e.g.*, motivation and emotion) and been defined in virtually the same terms. The reader might then have been provoked into marveling at the sheer amount of unification which behavior theory produces; or, contrarily, he might have been led to wonder whether all this unity is not after all achieved at the cost of ignoring some important psychological facts.

A second and more important inconsistency has to do with the term in

Doob's definition which asserts that an attitude is a response. Elsewhere (p. 143), however, he writes that, "The individual . . . may not express his attitude in overt behavior because its expression would be contrary to his general philosophy; *but his attitude persists*" (italics added). Now, by any ordinary usage of the word 'response,' a response occurs and is gone; it does not persist. In other words, if an attitude can persist, it cannot be a response. Doob may, of course, have in mind a class of persistent responses, but such responses do not seem to belong in the stimulus-response formula and, hence, in behavior theory. In all S–R psychologies one looks for a stimulus which immediately precedes the response, and to admit responses which keep going (perhaps for many years) is certainly to change the psychological significance of the stimulus term in the S–R formula.

Despite his definition of an attitude as a response, Doob often seems on the verge of thinking of an attitude as a habit or an established stimulus-response 'bond' (*cf.* pp. 141 ff.). Thus he defines the strength of an attitude in terms of the strengths of the stimulus-response bonds in which it is involved. This would give meaning to Doob's statement that an attitude may persist, for, while the response may be momentary, the bond presumably persists. It may be more sensible, therefore, to think of an attitude as the *habit* rather than as the response.

But an attitude cannot be both a response and a habit; it must be one or the other or neither. It is our inclination to say neither. For in customary usage it seems that there may be both persistent and momentary (possibly but not necessarily, recurrent) attitudes. And it seems to us that both have more in common with each other than either does, respectively, with a habit or a response.

Closely related to the question of whether an attitude is a response is the question of the relation of attitude to set. In our own conception, attitudes are a species of set. Doob also concedes (p. 139) that "Attitude may be included among the sets determining both the orientation of the individual as well as the kind of perceptual response he makes." Sets may also be momentary or persistent (and hence our previous question as to whether an attitude is best conceived of in relation to habits or responses or to neither applies to set as well), but, even if we concede that all sets are responses, the important thing about a set is not that it is a response, but the selective function that it plays. In other words, in playing up the response aspect of attitude, Doob is missing the boat.

That Doob seems to have some kind of antipathy to set is suggested by the following passage (p. 139): "Writers on attitude are fond of recalling that the tradition of set is somewhat hoary in academic psychology, in order to demonstrate their own respectability and their acquaintance with long German terms like *Bewusstseinslage*." Perhaps this antipathy accounts for Doob's insistence that an attitude is implicit, *i.e.*, not "overt behavior that is observable to an outsider" (p. 136). His sole justification for the criterion of implicitness is his opinion (p. 136) that, "The semantic connection of such sets [motor 'attitudes' like the physical set of the runner before the starter's gun is fired] with the concept of attitude employed by social scientists is . . . largely fortuitous from an historical standpoint." But if attitudes are a species of set, then the connection is by no means an historical accident; because, apart from the question of whether any set can be said to have no motor aspects, sets

which are manifestly motor and other sets have much in common, at least in a functional sense.

The matter is of some importance because, in making implicitness a defining attribute of attitude, Doob introduces a quite dispensable source of ambiguity. There is, of course, no sharp line of demarcation between *implicit* and *explicit* as Doob defines these terms; observability varies in degree. This implies that it must in many instances be difficult to decide (by Doob's definition) whether or not one is dealing with an attitude. How unobservable must an attitude be before one ceases to refer to it as an attitude? Also, it must often be difficult to distinguish between an overt *manifestation* of an attitude and an overt *consequence* of it. For instance, is a *moue* of distaste a manifestation or a consequence? If it should be decided that it is a manifestation, then, by Doob's definition, whatever it is a manifestation of *is not* an attitude; but if it is regarded as a consequence, then what it is a consequence of *is* an attitude. Insofar as we may arbitrarily regard it as either manifestation or consequence, then we may arbitrarily decide that we are or are not dealing with an attitude.

As the final item in this section we shall raise the question of whether an attitude is properly defined as anticipatory. Doob follows Hull in defining (p. 136) an anticipatory response as "one which originally preceded another rewarded response and which, as a result of being associated with or producing this reward, has been reinforced so that it occurs before its 'original time in the response series.'" Doob's illustrations are singularly unilluminating with regard to attitudes as anticipatory responses. Thus, in the case of an individual disliking another person, we are told (pp. 136–7), "Originally the avoidance occurred only after actual contact had been established and after the contact had proven to be punishing and the withdrawal to be rewarding." It is clear that the avoidance response may occur more quickly as a result of learning than it did originally, but it is not clear how it can be regarded as an anticipatory response since, being the goal response, it is always necessarily the last in the series. Doob, as a matter of fact, explicitly states that an attitude precedes (leads to) the goal response. Perhaps, of course, the attitude is the dislike rather than the avoidance (the latter being regarded, perhaps, as a response to the former) and it is the dislike which moves up in the response series. The dislike presumably (at least by the definition) does not become an attitude until it does move up in the response series, but it is not at all clear why a dislike which arises more quickly should be called an attitude while one which arises less quickly (and is in fact the same dislike) should not be called an attitude. Moreover Doob goes on to say (p. 137) that, "When a thorough investigation reveals no actual prior contact, some process of generalization or discrimination must have occurred since all behavior has antecedents." In other words, not only does an attitude occur before its original time in the response series, but it seems that it may occur in a response series in which it has never previously occurred at all; if mere temporal placement may define anticipation, then this is anticipation with a vengeance.

Again the matter is of some importance because making an attitude, by definition, anticipatory, leads to various difficulties. It precludes, for instance, the possibility of functional or dynamic contemporaneity between the attitude and the pertinent goal response. Even if an attitude may be said to persist, Doob's position implies that whatever effect an attitude may have has already

taken place by the time the goal response occurs. Yet, to take one example, a young man may have an attitude about kissing girls even while acting in accordance with, or contrary to, his attitude; and his enjoyment of the process and later reactions to it may continue to be influenced by his attitude. Doob's formulation raises the problem which confronts all attempts to deal with dynamics in historical, as distinguished from ahistorical (*cf.* 2) terms, namely, how something which no longer exists (and it no longer exists because it antedates its effects) can influence something which is now going on.

IV. Points Not Required by Behavior Theory

In this section we shall concern ourselves with several items of Doob's conception which may not be required by behavior theory. In other words, they may be dropped or possibly modified without doing violence to the latter. At the same time it should be remembered that they were developed in connection with behavior theory and the weakness involved may be applicable elsewhere.

Doob's definition requires that attitudes play a mediating role and he explains (p. 137) that "A mediating response is made in an attempt to increase the likelihood of the occurrence of reward rather than punishment in connection with a goal response." This qualification imposes, without any apparent justification, a restriction upon the concept of attitude which is not in accord with current usage. And while we cannot discern any reason for the restriction, there are several reasons for objecting to it.

In the first place, attitudes may *decrease* the likelihood of goal responses being rewarding. For instance, a student who cheats on an examination may find his 'achievement' rather empty as a consequence of his attitude regarding honesty. In the second place, by defining attitudes as mediating, they are functionally placed *between* the person and his goal responses. Yet attitudes play a role in the very selection of goal responses. For example, partly as a function of his attitudes concerning personal integrity, a student may not have the alternative of handing in a term paper which he himself has not written. It is misleading to say that such attitudes are attempts to increase the likelihood of reward in connection with goal responses when, actually, they play a role in defining what is rewarding.

Moreover, by emphasizing goal *responses,* Doob imposes an unnecessary and irrelevant restriction upon motivation and, thereby, on the relation of attitude to motivation. There are many motives which do not have goal responses or to which goal responses are essentially irrelevant. Thus Lewis (5) has demonstrated that for task-oriented (as distinguished from ego-oriented) subjects the important thing is that a desired state of affairs be achieved, regardless of who accomplishes it; in such a situation the subject has a goal, but *he* does not necessarily have a goal response. The present writer has also argued (1) for the existence of an important class of motives (*perpetuated* motives) with regard to which there are no consummations and, consequently, no goal responses. In general any motivation to 'be prepared' belongs in the general class of motives which do not necessarily have goal responses.

Doob may, of course, reply that there are indeed goal responses in connection with such motives, namely, perceptual responses. It is the perception that the task has been completed that reduces the tension. This reply is itself, however, not unobjectionable. To be consistent, the reply applies to virtually all motivation; for one can reasonably argue that no attainment of a goal is

satisfying without awareness of the attainment. In other words, according to this view, the goal response is the awareness of achievement rather than the achievement itself. But such a conception makes it impossible to set up objective criteria of goal-seeking as Holt and Tolman have done (4, 7). The criterion of tension-reduction is not sufficient, for there are different types of tension-reduction. For instance, a man running for a train may stop running and relax when he realizes that he is too late. The realization may be tension-reducing, but it is not a goal response in any ordinary sense of the term, and it certainly involves a different mechanism of tension-reduction from catching the train. One may reasonably ask, however, whether it is the perceptual response (the act of perceiving) which is tension reducing or the state of affairs which is perceived (the content of the perception). The latter alternative not only seems to us to be the more sensible, but it gives full recognition to the importance of the awareness of achievement without making it impossible to provide objective criteria of goal-seeking. On this basis, however, the only possible meaning of 'goal-response' involves doing something to bring about a particular state of affairs, rather than perceiving it. And we are back to our original contention that there are many motives without goal responses.

As a second item we shall deal with Doob's analysis of attitude strength. Despite our differences with this analysis, we regard it as an extremely important contribution to the psychology of attitudes, for it makes some important and hitherto neglected distinctions. It should be noted, therefore, that our objections are not to the distinctions but to the framework of behavior theory within which they are made.

Doob distinguishes between *"afferent-habit strength* of the attitude," its *"efferent-habit strength,"* and its *"drive strength."* He defines the first (p. 141) as "the strength of the bond between the stimulus pattern and the response which is here defined as attitude"; the second (p. 142) as the strength of the "bond between attitude as a stimulus and a response pattern (implicit or overt), rather than between a stimulus pattern and attitude as a response"; and the third he characterizes (p. 142) as follows: "An attitude has cue-value in the sense that it acts as a stimulus to produce another response, but it also is a drive in the sense that its tension is reduced through subsequent behavior leading to a reward." While he never quite defines "drive *strength,*" it seems clear that this term signifies the degree of impulsion to tension reducing behavior.

Afferent and efferent habit strengths are both described in terms of the strength of stimulus-response bonds. What Doob means by the *strength* of such a bond is indicated when he writes (p. 142), ". . . it may not be frequently evoked (a weak afferent habit). . . ." The translation from *strength* to *frequency* of *evocation* is illuminating. Mere frequency of evocation is obviously irrelevant except in relation to the frequency of occurrence of the stimulus pattern. But if it is evoked once by a given stimulus pattern, why not all the time? To answer this question in terms of habit strength is sheer tautology. The obviously correct answer is that conditions (inner and outer) surrounding the bond are different from time to time. It follows that the relative frequency of evocation is a function of the surrounding conditions rather than of "the strength of the bond" and that, in this respect at least, all bonds are equally strong.[4]

[4] It may, of course, be argued that Doob's translation of bond strength into frequency of

If 'bonds' may be said to differ in strength, the strength would have to be measured in terms of what it takes to disrupt them rather than in terms of frequency of occurrence. But if the amount of effort needed to undo a 'bond' is a measure of its strength, it follows that its strength is a function of the knowledge, skill, strategy, and/or appropriateness with which these efforts are marshalled against it. It becomes apparent that its strength does not reside solely in the bond. This does not mean that such strength is scientifically insignificant, any more than *mass* is insignificant because it does not inhere in physical objects. It does mean that in dealing with the strength of attitudes one's perspective has to take in more than the learning process and the 'bonds' themselves.

With regard to "drive strength," it is at present a moot point whether the 'drives' associated with some attitudes come from the attitudes themselves or whether the motivational force comes from factors beyond the attitudes themselves. The present writer is inclined to favor the second of these possibilities. One may find attitude and drive varying independently. Many of one's attitudes toward food may, for instance, remain unchanged as one goes through the hunger cycle. One may have an attitude with regard to, say, the marital difficulties of two relative strangers and what seems to be the same attitude with regard to identical difficulties between one's two closest friends. Does the lack of impulsion to do anything about it in the first case as compared to the strong impulsion to do something in the second case signify that these are really two different attitudes of different drive strengths? Or does the drive in the second case derive from one's concern about one's friends, the attitude to the marital difficulties being in both cases the same? Or, to take another instance, the attitude of regarding one's own ethnic group as superior to other ethnic groups,—does this attitude derive its motivational force (*e.g.*, to resist any overt contradiction of the attitude) from within itself, or does it derive its

evocation was merely a careless slip and that it does not do justice to behavior theory. If bond strength can be defined independently of frequency of evocation, then it may perhaps be said that the latter is a function of both the bond strength and conditions surrounding the bond. Such a definition may apparently be found in terms of the frequency (and quality?) of the reinforcement involved in establishing the bond. We say that it may *apparently* be found because, actually, the effect of such a definition is to make "bond strength" a verbal synonym of "frequency of reinforcement." One may, perhaps, argue that such a definition is really a hypothesis that (a) there really is such a variable as bond strength, (b) it is a function of frequency of reinforcement, and (c) it may be measured in terms of the latter. Unless, however, some way is presented to verify (a) and (b), the hypothesis is empty and (c) simply establishes "bond strength" and "frequency of reinforcement" as verbal synonyms.

A test of the hypothesis might be provided by showing that the relative frequency of evocation varies as a function of frequency of reinforcement despite the constancy of surrounding conditions. Unfortunately, we have no way of holding the inner conditions, at least, constant. Let us, however, grant for purposes of discussion the successful performance of such an experiment, with positive results. Such results would require a peculiar assumption to explain them. Since all surrounding conditions are being held constant, variation in frequency of evocation could only be a function of *variation* in the bond (because for a given status of the bond it either will or will not produce the response under given stimulation). And this variation in the bond cannot be a function of some variable factor acting on the bond, since it is agreed that all other factors are constant. In other words, we would have to assume that the bond has acquired an inner life as a result of which it waxes and wanes independently of what is going on around it. We might then argue whether such a waxing and waning is best described in terms of an invariant adjective, a-given-degree-of-strength. But, for our own part, we would rather not make this assumption until we absolutely have to.

force from such motives as the need to maintain one's level of self-esteem?

We have already stressed the point that our objections to Doob's distinctions between the three types of "attitude strength" are not to the distinctions themselves, but to the conceptual framework within which they are made. The distinctions have two definite values. In the first place, they throw light on what may happen when ratings of the strengths of attitudes are made. Roughly corresponding to Doob's "afferent-habit strength," these ratings may reflect the degree of confidence that the attitude actually exists (*e.g.*, ratings of low intensity may correspond to "Sometimes I feel this way and sometimes I don't—I'm not sure how I feel!" Or, "I am ambivalent about it—I feel both ways—I'm not sure how I feel"); or, roughly corresponding to Doob's "efferent-habit strength," these ratings may reflect one's degree of conviction that something ought to be done and one's readiness to support such action; or, finally, roughly corresponding to Doob's "drive strength," they may reflect the intensity of one's urge to do something oneself. The implication is clear that, in obtaining ratings of the intensity of attitudes, one should if possible specify the dimension of intensity involved.

In the second place, the issue of attitude strength aside, Doob's analysis is valuable in that it calls attention to the fact that an attitude is not completely described until we know its perceptual-cognitive content (the object, action, or situation to which it is pertinent), its action implications (one's beliefs about what should be done about, or with regard to, the object), and its motivational force (the strength of the motives with which it is bound up). But one cannot separate the cognitive from the action aspects as Doob does when he treats "afferent-habit strength" as involving a different 'bond' from "efferent-habit strength." If two people have attitudes toward a common object, but differ on what they believe should be done with regard to that object, it seems both simpler and more accurate to say that they have different attitudes toward that object than to say that they have a common afferent habit and different efferent habits. Nor can one give unqualified assent to Doob's statement (p. 143) that "Afferent-habit strength depends ultimately on efferent-habit strength since reinforcement or non-reinforcement results not from the arousal of the attitude but from the behavior mediated by the attitude." For if one could disrupt the "afferent habit," the "efferent habit" would automatically go with it, there being nothing to arouse it. In other words, it is just as true to say that the efferent-habit depends on the afferent-habit. The two are, in fact, inseparable.

In passing, we may note Doob's remark (p. 144) that a stereotype "becomes an attitude . . . only when it is drive-producing." It seems to us that a better formulation would be that a stereotype is not an attitude because, taken by itself, it has no action implications (*i.e.*, it involves no opinion or belief concerning what should be done about the object of the stereotype). It is part of an attitude and may be used to gauge attitudes only insofar as one may assume action implications. In Doob's language, it is an afferent habit which does not necessarily have any efferent habits associated with it. Insofar as it creates a likelihood of accepting certain action implications, it is a condition of an attitude. Concretely one may think of certain people as being 'dirty,' but if one is indifferent toward people's dirtiness this is not an attitude. Associated drives, on the other hand, are not at all necessary to have an attitude. One may, for instance, have an

attitude toward the caste system in India without being at all impelled to do something about it.

For the last item in this section we turn to Doob's point concerning the social significance of attitudes. It is in this connection that Doob urges the point that attitude is not a sound scientific concept. An attitude, he maintains, is psychologically no different from any other learned response; that it is socially evaluated may make it practically important to distinguish an attitude from other responses, but this distinction has no psychological or scientific significance.

Even if Doob were correct at all other points, we must take exception to this conclusion. In view of the needs that people have to conform and to win social approval the fact that attitudes may be socially evaluated would give them great psychological significance; they would, in Doob's language, have a unique type of stimulus value. Nor can psychologists and other scientists dismiss social evaluations as being beyond the pale of science, even apart from the preceding point. Social evaluations are legitimate objects of scientific inquiry. They may legitimately be described; the conditions of their origins, transformations, and maintenance may be investigated; their effects may be studied; and scientists should have something to contribute on their soundness.

In the light of so vital an issue, it may seem trivial, but we must also object to the unwarranted restriction which the criterion of social significance imposes on customary usage. We do not see any sound reason for excluding attitudes on matters with regard to which society may be more or less indifferent. Conversely, we cannot agree that the criterion does differentiate attitude from other psychological phenomena; that a desire to pray, or to eat some forbidden food, are attitudes simply because they are socially evaluated. Doob's reason for the criterion is that he could find no other *differentium* of attitude, but we submit that this is a fault of his analysis.

V. Points Required by Behavior Theory

Doob takes Sherif and Cantril (6) to task for saying that it is possible to discuss attitudes without committing oneself to any particular learning theory. In a sense, a proponent of behavior theory has to take such a position because behavior theory is built around a particular learning theory. If it is to offer an interpretation of any psychological phenomenon, it must do so in terms of its particular theory of the learning process.

Yet there is good reason for not committing oneself to a particular learning theory in one's conception of the *nature* of attitude, especially if that learning theory is still debatable. A definition may itself be hypothetical (the hypothesis being that something exists which corresponds to the definition), but there is danger in saddling it with other hypotheses. Doing so creates a bias with regard to certain facts, for these facts are required by the *definition* whether they happen to be true or not. If an attitude can develop which is in no way different from what Doob calls an attitude, except that it has a different genesis, why exclude it? The only distinction would be genetic rather than either structural or functional. If, on the other hand, attitudes can arise in no other way than that postulated by Doob, the incorporation of the developmental theory in the definition is just so much excess baggage.

This objection applies as much to the specification that attitudes are learned as to the specification that they are learned in a particular way. The assertion that attitudes can only arise as a

result of learning may be a very interesting one, and worthy of full credence, but it can only be verified if we first know what an attitude is, independently of the assertion. Otherwise the alleged *fact* is established by an arbitrary definition, *i.e.*, by a verbal fiat. This is not supposed to be the way facts are established in science.

If our argument is sound, behavior theory cannot validly offer an explanation of the *nature* of an attitude (or of any other psychological phenomenon, for that matter, other than the learning process itself)—precisely because it is no more than a particular learning theory. It may suggest a hypothetical explanation of how attitudes arise, but not of what they are. And it cannot even begin to suggest how they arise (except in the form of the blanket hypothesis that all psychological phenomena arise in exactly the same way) until it has some notion of what they are, in the first place.

Concerning the genesis of attitudes, we have already indicated in presenting our alternative to Doob's conception of the nature of an attitude our agreement with the proposition that the learning process plays a major rôle. This is, by no means, complete agreement, for Doob admits only the learning process (and only a particular kind of learning process, at that). To hold the latter position, it seem to us, is to take insufficient account of creative cognitive processes, both of thought and perception. Such processes, we believe, also play a role in the generation of attitudes. It consequently seems to us that a more satisfactory formulation of the relation of learning to attitude would be: The residues of past learning play a rôle in determining how a given situation or object is perceived and what a person wants in the situation or with regard to the object. The latter, in turn, play a rôle in determining the attitudes which are generated and the salience of already existing attitudes. The important dynamic fact is how the person perceives the situation and what he wants in it rather than the fact that learning had previously taken place.

In this connection, we believe that there is an intellectual trap in the commonly accepted principle that if some psychological process is not learned, then it must be innate and, if it is not innate, then it must be learned. We believe that new insights do arise, and we see no good reason why novel features of current situations should not be perceived or why previously unnoticed features of repeatedly experienced situations may not be perceived for the first time. We also see no reason why such cognitive novelties should not play a significant rôle with relation to attitude despite an absence of associated learning. The atom bomb was something new to most people; they had no established responses to it; but it exercised immediate and profound effects on the attitudes of many of them.

In line with behavior theory, Doob takes the position (p. 140) that "Some stimuli come to arouse an attitude after a relatively simple process of conditioning has occurred. . . . Other stimuli evoke or fail to evoke an attitude not because of their presence or absence in previous situations but because they fall along a stimulus gradient of generalization or discrimination." The recognition of such gradients is of course a great improvement over the oversimplification of early conditioning theories, but from our point of view, this is still insufficient.

The proponents of the more sophisticated conditioning theories still act as if new perceptions never occur or, if they do occur, as if they have no dynamic significance since, being new, they are

not directly or indirectly associated with past learning. Suppose, for example, that a new perception occurs which just doesn't happen to fall along some generalization or discrimination gradient. Will a person remain completely unaffected by it? We think not. Nor can one argue that it is impossible for a perception not to fall along such a gradient without also assuming that these gradients are completely preestablished. Otherwise how can a new gradient ever come into being?

This point is particularly emphasized when these gradients are themselves interpreted as resulting solely from secondary conditioning. The Gestalt psychologists have presented enough evidence on structural processes in perceptual grouping to raise doubts as to the sufficiency of secondary conditioning in this context. It is necessary, therefore, to appeal to some supplementary principle as Doob does when he turns (p. 141) to similarity as a factor involved in producing generalization gradients. But if similarity is interpreted in terms of a doctrine of identical elements or identical neural pathways, the viewpoint is again committed to the assumption that there can be no novel perceptual processes, an assumption which we are not prepared to accept. If, on the other hand, similarity is interpreted in other terms, then something more is involved in the generalization gradients besides the learning process itself.

VI. CONCLUSION

1. At various points in the preceding discussion we have challenged virtually every term of Doob's definition and much of his analysis. We are aware that in many instances the challenge amounts to little more than a contrary assertion. For example, while we appealed several times to current usage, we made no attempt to show that our interpretation of current usage is superior to Doob's or that the referent of current usage is real and validly distinguished. Nor have we attempted to show that our alternative to Doob's conception of attitude is not open to as many objections as we have found in his. We have assumed that, in such instances, the reader will determine for himself how to side or whether to suspend judgment. Our main concern has been to keep the discussion going and not to permit any of the issues to go by default. At stake is not the definition of a word, but the definition of a whole area of psychological inquiry.

We should add that we would be greatly surprised if our own everyday usage of the word 'attitude' differed significantly from Doob's. The difficulty is not in our "knowledge of acquaintance" with attitudes, but in the effort to translate that knowledge of acquaintance into 'knowledge about.'

2. If our criticisms are sound, Doob has not offered an acceptable interpretation of the concept of attitude, he has not succeeded in assimilating it to behavior theory, and it has yet to be demonstrated that behavior theory can assimilate or replace the concept of attitude.

3. Since some of our criticisms concern fundamental points of behavior theory, then, if these criticisms are sound, one may question whether behavior theory can assimilate the concept of attitude.

4. It would be overhasty to abandon the concept of attitude on the basis of Doob's argument.

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